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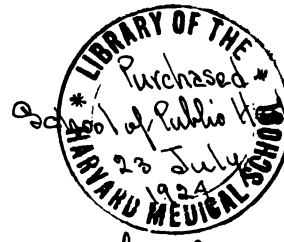

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1863. Rowe, T. Smith, M.D., 1, Union Crescent, Margate.
1881. Russell, E. G., M.B., B.Sc., Lieut.-Col., Bengal Army.
1859. Sanderson, J. Burdon, M.D., F.R.S., 64, Banbury Road, Oxford.
1858. Scratchly, G., New Orleans, U.S.A.
1884. Simpson, W. J., M.D., Bengal Club, Calcutta.
1888. Smith, F., Montreal, Cargate Avenue, Aldershot.
1867. Smith, R. Wagstaff, Mount Rundell, Harborne, Birmingham.
1854. Spinks, C. N., Sankey Street, Warrington.
1889. Street, A. F., M.D., St. Mildred's Road, Westgate-on-Sea, Kent.
1885. Sturridge, P. F., Kendal, Westmorland.
1880. Tatham, John, M.D., Somerset House, W.C.
1894. Tebb, W. Scott, M.D., Boscombe, Bournemouth
1853. Todd, J. George, Evenwood, West Auckland, Durham.
1885. Tomes, Arthur, Surg.-Major Ind. Med. Service, Bengal.
1873. Townsend, Stephen Chapman, C.B., Surg.-Gen.Ind. Med. Dept., Sanitary Commissioner for Central India.
1879. Watson, Alexander, M.D., R.N., Insp.-Gen., Army and Navy Club.
1880. Whishaw, J. C., M.D., Surg.-Major Ind. Med. Dept., Lucknow.
1893. Whiston, P. H., Surg.-Capt., The Palace, Rochester, Kent.
1880. White, J. Berry, M.D., Surg.-Major Ind. Med. Dept. (*address unknown*).
1854. Wiblin, John, The Hermitage, Clewer, Windsor.
1875. Wortabet, John, M.D., St. John's Hospital, Beyrout.

Corresponding Members.

1875. Arkhangelsky, D., M.D., St. Petersburg, Russia.
1852. Barker, R. H., Hungerford, Berks.
1852. Bartolomé, Mariano Martin de, 334, Glossop Road, Sheffield.
1856. Barton, E. H., New Orleans, U.S.A.
1853. Beneke, F. W., Marburg, Germany.
1860. Brionde, A. de (*address unknown*).
1851. Cartledge, Benjamin, Vet. Surg., Sheffield.
1894. Catrin, L., Dr., Val-de-Grâce, Paris.
1871. Christie, James, M.D., Zanzibar.

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TRANSACTIONS

OF

The Epidemiological Society of London.

INAUGURAL ADDRESS OF SESSION 1894–95.

On the Study of Epidemiology.

By SHIRLEY F. MURPHY, President.

(*Read: November 16th*, 1894.)

Gentlemen,—It is my first duty to thank the members of the Society very sincerely for the honour they have done me in electing me their President. The roll of Presidents contains the names of many of the most distinguished members of our profession whose labours have added to our knowledge of disease, and especially of the particular class of disease which this society was founded to study. I fully recognise, therefore, that it is no slight honour to succeed those who have in the past shed lustre upon the society's proceedings, and I am none the less conscious how greatly it adds to the responsibilities of one who, by your favour, is called to the presidential chair. Not the least of these responsibilities is the choice of a subject which may fittingly engage your attention on the occasion of the first meeting of the session, for, thanks to the activity of our secretaries, each topic of current interest is brought to our notice during the ordinary meetings of our society, and where the harvest is so well gathered it is no easy task to find a portion of the field which has not already yielded to you its fruits. I may, perhaps, be pardoned if, under these circumstances, I content myself with a brief reference to the intentions of the founders of epidemiological study and indicate the position of our knowledge in respect to a few of the conditions which affect epidemic disease.

The study of epidemiology has been strongly urged by Hecker in 1832. In his Address to the Physicians of Germany, with which we are familiar in connection with his work on the Epidemics of the Middle Ages, he stated

his desire to "impress on them a subject in which science is deeply interested, and which, according to the direct evidence of nature herself, is one of the most exalted and important that can be submitted to the researches of the learned." "I allude," he said, "to the investigation of epidemic diseases on a scale commensurate with the extent of our exertions in other departments, and worthy of the age in which we live." He contrasted the scientific study of individual diseases, and the success which had attended it, with the neglect of the study of the "disease of nations", and pointed out that "the doctrine of epidemics surrounded by the other flourishing branches of medicine remains alone unfruitful, we might almost say stunted in its growth." Hecker's demand for the study of epidemiology did not long remain without response in this country. The need for such study had been emphasised by the disaster inflicted upon the English population by the cholera epidemics of 1832 and 1849, and the time was more than ripe for more systematic study of the conditions which fostered disease. Of the past there was much historical record showing the progress of disease from one country to another, showing also the association of unwholesome conditions with exceptional prevalence of epidemic maladies, but wanting more exact definition of the relation of each of these unwholesome conditions to the spread of particular maladies. Further, there remained the need for explanation of the behaviour of certain epidemic diseases not, so far as was known, constantly present in our midst; explanation of the circumstances which endowed an Asiatic disease like cholera with power to visit Europe from time to time, while it absented itself in the intervals, or which at times gave to an endemic disease like small-pox a new power of dissemination, enabling it after certain intervals to exercise a far more destructive influence in one period than in another. That such occurrences did not escape observation the literature of the past abundantly shows. They afforded, as they do still, ground for speculation, but the material for their exact study did not at the time exist. It was this need which the founders of our society felt, and for them there was encouragement in the fact that England had, among other countries, instituted a system of registration of deaths and causes of death, the necessary foundation of exact epidemiological inquiry.

At our social gatherings Sir Benjamin Ward Richardson has told us of the beginning of our society, of the thoughts possessed by those who were its founders. For our part

we look back upon those early days with gratitude, and with appreciation of the foresight of those who at that time saw the possibility of the acquisition of a fuller knowledge of epidemic disease. For their use they had the history of the past, a history full of interest, but involved in much obscurity, and useful rather in giving incentive to the study of epidemic disease than in affording material for explanation of its phenomena; for the study of existing disease in this country they had the old Bills of Mortality and some thirteen years of the better records of the Registrar-General. There was ample field in the study of the geographical distribution of disease, its altitudinal range, the character of the soil of the localities where it prevailed, the climatic conditions which appeared to foster it, and the character of the population which is most infected; there was, moreover, the important question of the means by which it was disseminated, and the speculation whether this was brought about by some contagium produced in the soil, or in the bodies of the sufferers, or in the animal or vegetable life with which they are surrounded. How broad a view the society has always taken of the work before it is shown by the admirable "Objects Paper" of the society, which indicates the subject matter demanding the study of the epidemiologist. In this excellent statement we are told that the epidemiologist must include in his studies the great group of idiopathic fevers, the sanitary condition of places and dwellings, topographical medicine, the geography of epidemic and endemic diseases, the chronology of epidemics, and he must also obtain knowledge concerning epizootics and epiphytics. It is clear, therefore, that the society, while recognising the necessity of studying the behaviour of human disease in respect to time and place, was in no way disposed to limit its studies to the human animal in seeking explanation of the circumstances in which such disease is caused.

Some ten years after our society was instituted Professor Hirsch produced his admirable work on *Geographical and Historical Pathology*, which he dedicated to the society. He states in his preface to the second edition of this work that his task had been "not merely to collect and reduce to order an almost unmanageable heap of materials—for the most part a *rudis indigestaque moles*—and to test critically their authenticity and fitness, but it more particularly involved the founding according to a design, and the building up according to a system, of a discipline which had been the subject of but little labour before; and had

still to make good its right to a place among the medical sciences." Further, he felt himself able to claim that his first edition had "given an impulse to the further cultivation of this field of medical knowledge", and "that the progress made within the last thirty years by this discipline with the decided influence on the course of etiological inquiry, and its infusion into it of a more scientific character", had been connected in no small degree with the publication of his book. Professor Hirsch had, in fact, culled from all sources the material required for his purpose, he had digested this material, and had, so far as he could, apportioned to each statement the value which attached to it. The reader of his book is thus able to appreciate the "discipline" to which he refers. The choice of this word by Dr. Creighton, who has thus translated Professor Hirsch's own word *Doctrin*, is excellent, for it embodies the very essential of epidemiological inquiry. Professor Hirsch has necessarily been obliged to rely very much upon the observations of others in the preparation of his work. He groups the subject matter which he thus collects under certain well-defined headings; he gives such account as he can of the past history of the disease; he discusses at length its geographical distribution in our own times; he contrasts the peculiarities of epidemics of the disease under consideration with those of epidemics of other diseases; the influence of seasons and weather is especially dwelt upon. The influence of soil, the difference in the type of the disease at one time and at another time, the nature of the virus, and the method of the diffusion, are all subjects he discusses separately. He has shown (to use his own words) a design, a discipline, which must be followed by the student of epidemiology.

Our society may properly claim that its members have not been unmindful of this necessary discipline. Whether in papers read at our meetings or in investigations conducted in their several official capacities, they have demonstrated that they are working on intelligible lines. It is right that we should recognise how much the progress which has been made is due to the methodical inquiries into the circumstances attending disease prevalence which have been conducted by the medical staff of the Privy Council and Local Government Board. These investigations have provided definite object lessons for epidemiologists throughout the country. They have indicated the subject matter to which attention should be directed and the method of its investigation; they have enabled dis-

tinction to be made between conclusions based upon the accurately observed behaviour of disease and generalisations which are based on insufficient data. It is not often, however, that the work of the investigator immediately supplies material for the formulation of conclusions. Usually the truth is only appreciated after a series of observations, each of which gives some indication well deserving of record. We must, therefore, be prepared to recognise that our matter is often only suggestive—is, perhaps, sufficient only for provisional hypothesis; and so long as we abstain from attaching to it a greater weight than it deserves we may with advantage submit it to the criticism of those who with ourselves are endeavouring to solve the same problem. It is such a purpose that our society is best qualified to serve, and in proposing to submit for your consideration a brief account of a few of the circumstances that appear to influence the behaviour of epidemic disease I shall not hesitate to include certain indications supplied by the London records of notification of infectious disease.

I desire in the first instance to refer to seasonal changes in the prevalence of epidemic diseases. Much skill has been shown in designing methods for studying the relation of disease prevalence to meteorological conditions. In the beginning of 1875 Mr. Alexander Buchan and Dr. Arthur Mitchell presented to the Scottish Meteorological Society an important paper in which was shown in diagrammatical form the weekly excess or defect of mortality in London from different diseases in relation to the mean of thirty years. The results thus obtained enable comparisons to be made between the mean mortality and the mean meteorological conditions of each week in these thirty years. In 1878, dealing with the subject in the same manner, comparison was made between London and New York, and the mean meteorological conditions of the latter place were given. The one city can therefore be compared with the other in respect of their mortality and meteorology, and broadly the meteorological conditions which are related to excess or defect of the disease can be appreciated. No one who studies the diagrams for these two cities can fail to be struck with the fact that while, broadly, the "curves" for small-pox, diphtheria, diarrhœa, and typhoid fever are generally similar in London and New York, that of scarlet fever is very different. The mortality from this disease, which in London is below the average from January to July, is above the average in New York from January to June, and while in London above the average from

August to December is in New York below the average from July to November. Again, while London suffers from excess of mortality from measles in the months of April, May, June, and July, and again in November, December, and January, the excess in New York is limited to the first seven months of the year. It has, indeed, but one period of excess instead of two, as in London. There is, however, a depression in the New York curve in March and April, which, if exaggerated to a defect, would give the disease two maxima as in London. Whooping-cough has in New York an autumnal excess as well as a spring excess, whereas in London whooping-cough has only a spring excess. Diarrhœa has, in both cities, curves resembling each other, the summer excess occurring some weeks earlier in New York than in London, and so again the enteric fever maximum of New York is earlier than that of London. Dr. Thomas Borthwick of Adelaide, in an interesting *Contribution to the Demography of South Australia*, published in 1891, has given the seasonal curves of mortality of the principal zymotic diseases in that part of the world. The numbers on which the curves are based are small, but they are, notwithstanding, interesting for the purposes of comparison with those of London. Diarrhœa behaves much as it does in London, and occurs especially at the hot season of the year; the excess is, however, of longer duration. Enteric fever, too, occurs as in London, if we allow for the difference in seasons. Whooping-cough corresponds with the London curve in having one excess, and to this extent differs from that of New York; but the maximum occurs two or three months later if the South Australian and London year be made to correspond in respect of season. The measles curve more nearly approaches the London curve in having two periods of excess, but measles in South Australia declines to be influenced by the difference between London and Australian seasons, and behaves more as if its prevalences were determined by other conditions which are synchronous with those of England and independent of temperature. Scarlet fever has its maximum somewhat later in South Australia, if allowance be made for difference of season; both in London and in South Australia it prefers for its full development a moderately warm temperature of about 50° F.

Turning to the records of the Registrar-General of England, we find, in his annual report for 1884, material enabling comparisons (though for periods of different

extent) to be made between the seasonal mortality from measles in London, in three groups of other English towns, in Scotch towns, in Paris, and in Berlin, and it is seen that the winter excess of London is only shared by two groups of the English towns, and by none of the others. Again, taking from the reports of the Registrar-General the deaths from scarlet fever in each week for six years in various Continental towns, it may be seen that while Stockholm and St. Petersburg have their greater mortality in the first quarter, Vienna and Paris in the second, and London, Berlin, and Budapest in the early weeks of the fourth quarter, Berlin and Budapest agree in having their lowest mortality in the first quarter, and are closely followed by London; Stockholm and St. Petersburg have their lowest mortality in the third quarter, and are closely followed by Paris and Vienna. Such agreements and differences are naturally associated with climatic conditions, but as soon as we begin to investigate the influence of such conditions on the prevalence of epidemic disease, we realise the complexity of the problem we are endeavouring to solve. We accept at once that diarrhœal prevalence is intimately related to temperature; we are disposed to accord to temperature a power of influencing some other of the diseases of this class; moisture of the air is also no doubt a factor which must be considered, and more recently reason has been given us for including the effect of sunlight among the possible or even probable conditions.

How great are the difficulties with which we meet when studying climatic conditions may be appreciated by reference to a recent work by Herr Körösi of Budapest, who has investigated the effects of temperature and moisture of the air upon the number of attacks of several zymotic diseases in Budapest. He says: "The phenomena to be observed (in this instance the number of cases) must, as in experiments, be exposed to the supposed causes, and then studied with a view to ascertaining whether with decreasing or increasing strength of the causes the effect (number of cases) increases or decreases. Such conclusions are the more reliable if the increase and decrease of the causes follow some system, so that the strength is altered by gradations; the more numerous the steps of the causal ladder, and the greater the constancy of the results, so much the more forcible is the proof of the cause." Körösi's method is to divide the whole time under observation into periods, and to calculate the number of

attacks in each; he then combines into the same group all the periods in which the causes behave with equal intensity, and thus obtains a number of groups which he arranges in order based upon the intensity of the causes, with a view to learning whether the number of cases varies with the degree of intensity of the cause. This method of grouping, he points out, unites periods chronologically far apart, and he states that as regards the physical or chemical action of cold or heat it is of no consequence whether this is observed in spring or in autumn or in one decennium or another. If warmth or moisture were the only causes which influence these diseases, he contends that according as these causes were stronger or weaker the effects would be greater or less. He recognises, however, that this hypothesis is untenable and that other influences are at work, and he claims, therefore, that it is enough if his method gives indications. He points out a further difficulty—that it is possible prevalence may be dependent on only one factor, the other being merely a concomitant. If a particular disease increases during very dry weather it may be really due to conditions of temperature, because the hottest days are the driest; he therefore selects periods when the moisture is uniform and the temperature variable, and, again, periods when the temperature is uniform and the moisture variable, and observes the number of cases in each period.

Körösi gives the following results of his investigations of diphtheria, scarlet fever, and measles during the ten years and a half, 1881 to 1891.

Diphtheria.—Most diphtheria cases occurred in times of moderate temperature. Great cold, equally with great heat, seems to hinder the spread of the disease. Nothing definite can be said as to the influence of moisture in the air, but it is probable that in the dampest days the number of cases increases. Moderate temperature, especially in damp weather, caused the greatest increase of cases.

Scarlet fever.—Generally speaking, with a rising temperature there was an increasing tendency for the cases to become more numerous. A mean temperature was most favourable to the spread of the disease, while the coldest days were most unfavourable. In cold, moderately warm, and warm days increasing moisture likewise aided the disease; but this was not found to be the case in the hottest weather, consequently Körösi regarded the influence of moisture as uncertain. The greatest number of cases

occurred in moderately warm and at the same time very damp weather.

Measles.—The investigation gives but little information as to a positive influence of temperature and moisture on the behaviour of measles. The wettest and at the same time the coldest days have shown themselves most favourable to the spread of measles. It is possible, though not yet finally proved, that with an increase of temperature and a decrease of moisture there may occur a decrease of the disease.

I have dwelt upon Körösi's work because it is that of an able statistician who has applied a particular method to the complex subject-matter which the epidemiologist has to examine in any attempt to learn the relation of these conditions to the prevalence of the diseases with which we are concerned. This method would, I believe, eventually afford a basis for comparing the effects of temperature and moisture on such prevalence; but the difficulty which attends it is probably that the other factors concerned in this prevalence exercise such an overpowering influence that it would take very many years before the factors of temperature and moisture could make their influence manifest. This is, of course, no reason for distrusting the method, but it is reason for postponing our conclusions until the material which is thus examined is sufficient to overwhelm such disturbing elements as prevalences due to other causes. Nevertheless, the statistical indications of Körösi's work—and he himself regards them only as indications—are to this extent of undoubted value.

Dr. Longstaff approached the same subject in another way. He showed diagrammatically the annual percentage excess and defect of several diseases in relation to the mean of twenty-five years, and he showed in the same way for each of these years the percentage variation of both the rainfall and the number of the rainy days, and added them together. These conditions can therefore be compared with the amount of prevalence of the disease, with the result that a very close correspondence between the two is observable. These meteorological conditions and the prevalence of pyæmia, puerperal fever, erysipelas, and rheumatism of the heart are, speaking generally, in inverse proportion; and to the extent of a faint echo there is some suggestion that laryngitis, quinsy, croup, diphtheria, and cynanche maligna are subject to these influences. It cannot be said that there is more than a general tendency in this direction, but the correspondence is such as to give

the impression that, while other conditions are also concerned in these prevalences, rain cannot be entirely set aside. Dr. Astley Gresswell, in his "Contribution to the Natural History of Scarlet Fever", observes the same inverse relation between rainfall and scarlet fever prevalence; he showed the records of both in each of the years 1862-87, and also in groups of three or four years, and thus compared the rainfall in different years and different groups of years with the amount of scarlet fever mortality. He found the rainfall and scarlet fever prevalence in inverse proportion, and comments on "the constancy with which scarlatina mortality has risen in dry and fallen in wet years; when the rainfall has been very heavy, scarlatina mortality has been low, and when the former has been very light the latter has been high." Gresswell also found that scarlet fever diminished during the first of a series of wet years, and in some cases it still further decreased during the second; hence, he says, "it may be suggested that not only the rainfall of the year, but also that of prior years, has influence on scarlatina." Gresswell's observations are not to be regarded as antagonistic to those of Körösi, who himself considered the influence of moisture as uncertain. Further, Gresswell recalls to our minds that rainfall does not stand alone among meteorological elements influencing scarlatina prevalence, and that there are many others in relation to it, such, for instance, as want of bright sunshine.

The effect of light upon the prevalence of communicable diseases demands more detailed study than it has yet received. It may be that the decrease of small-pox during the summer months is in part due to this condition, and certainly the recent observations of Dr. Arthur Ransome and Professor Sheridan Delépine on the "Influence of Certain Agents on the Tubercle Bacillus" are in this respect very suggestive. These experimentalists exposed tubercle bacilli to different conditions of light and air, and subsequently tested their virulence by the inoculation of guinea-pigs. I may state the results of their experiments in their own words: "So far as they extend at present they show (1) that finely divided tuberculous matter such as pure cultures of the bacillus or 'tuberculous dust' in daylight and in free currents of air is rapidly deprived of virulence; (2) that even in the dark, although the action is retarded, fresh air has still some disinfecting influence; and (3) that in the absence of air, or in confined air, the bacillus retains its power for long periods of time."

Numerous experiments have demonstrated the inhibitory action of light on the bacillus anthracis, and more recently Professor Marshall Ward has shown that the bactericidal action of the sun's rays is due to those in the blue violet half of the spectrum. There may, of course, be differences in the power of the several contagia to resist the influence of solar rays. Professor Marshall Ward has indicated such differences in the lower forms of vegetable life, and has associated them with the protection afforded by some colours against the chemical action of light upon some oxidisable body like a fat in the spore, the colour of the spore appearing to determine the ability of the fungus to grow in exposed places. In our study of the contagia we may expect to learn more of the action of light in determining the prevalences of the several epidemic diseases.

Social changes in the population may be expected to be reflected in the behaviour of epidemic diseases, and I therefore select to-night for consideration the results of the study of the effects upon communicable disease of that condition familiarly known to us as "school influence". The influence of school attendance on diphtheria prevalence at Brailes in 1876 was demonstrated by Mr. W. H. Power, and in the following year Dr. Thorne Thorne, in a report on diphtheria at Coggeshall, showed the special incidence of the disease upon children at the school-age period of life. In 1883 Mr. Power, in discussing a prevalence of diphtheria at Pirbright, in Surrey, pointed out differences in the behaviour of the disease in successive periods when the local school was open or closed. I do not propose to refer in detail to Mr. Power's elaborate analysis of the circumstances of this outbreak, but will content myself with saying that it was worthy of the investigator, and that it fully justified his conclusions that "no doubt" could "be entertained that conditions of school life played an important part in the speciality of the incidence of grave disease in children 3-12 [years of age] in Pirbright." Since attention was thus directed to the part schools can play in giving opportunity for the dissemination of infectious disease, there have been numerous records of such occurrences, and the question is properly raised whether, with increased facilities for education, which are necessarily accompanied by aggregation of children in schools, there has not been some manifestation of this change in our death returns. But should we necessarily at once find evidence of this change in increase of prevalence of disease? Later, I shall

refer to another factor in the determination of prevalence which might tend to obscure any influence which the change in question would exercise. Prevalence of disease, in fact, is due to other conditions than mere opportunities for diffusion, and we might not, therefore, immediately find any greater result than a difference in the incidence of the disease upon the several ages in periods before and after the change came into operation. In other words, we should expect a greater relative incidence of mortality upon children at the school-age period of life occurring after the larger aggregation of children in schools than before that event happened; but it might further be expected that this would be especially noticeable when for some reason the disease was showing a greater tendency to spread from person to person.

I have somewhat recently shown how far the statistics which are available support a hypothesis that children at the school-age period of life have in later years suffered proportionately more from diphtheria mortality than in earlier years. The story shortly told is this: The mean annual mortality from diphtheria in London in 1871-80 was less than in 1861-70, but the fall in mortality in 1871-80 was not equally shared by persons of the several ages. For some reason children from three to ten years of age shared less in this fall than children under three years of age or persons over ten years of age, suggesting that some new factor had come into existence which tended to maintain diphtheria prevalence among children from three to ten years of age. When the mean mortality from this disease in 1881-90 is compared with that of 1871-80, it is found that the mortality at all ages in 1881-90 has increased upon that in the former period, and further that this increase is greater for children at the ages of from three to ten years than for those under three or for persons above ten years of age. Again, if we compare the diphtheria mortality in London during 1891-93 with that of former periods, we find this especial incidence of the disease upon children from three to ten years of age, as compared with persons at other ages, still more plainly marked, the years 1891-93 being years of exceptional diphtheria prevalence. Thus, we note that the mean annual rate of mortality at all ages was in 1891-93 increased 313 per cent. above the mean annual rate of mortality in 1855-60; but while the increase of children under three years of age was 311 per cent., and above ten years of age 81 per cent., that of children from three to ten years of age was as much

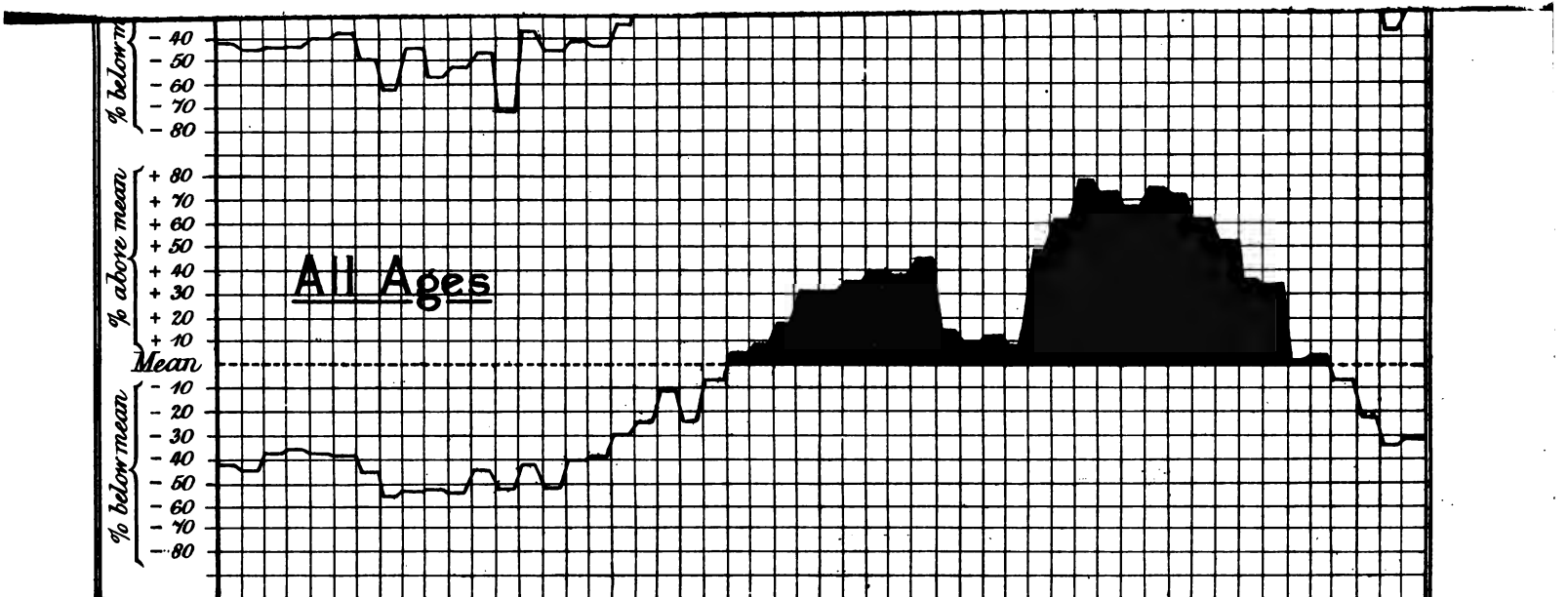
as 457 per cent. Again, if we compare the periods 1891-93 and 1861-70 in the same manner, we find that the increase of the rate at all ages in 1891-93 was 196 per cent. upon the rate of 1861-70, that of children under three years of age was 173 per cent., that over ten years of age was 53 per cent., while at from three to ten years of age the increase was 309 per cent. There is, therefore, at the present time a greater tendency of children from three to ten years of age to die from diphtheria than in earlier times, and this increased tendency was first manifested somewhere between the two periods 1861-70 and 1871-80. A closer examination of the age incidence of mortality from this disease in each of the years 1861-80 is therefore necessary, and such examination points to the year 1871 as that in which this increased incidence first became conspicuous. It is interesting to note in this connection that the Elementary Education Act first became operative in that year. The question necessarily deserves consideration whether alteration in nomenclature may be responsible for the behaviour of the figures which I have indicated, but it is found that the inclusion of croup with diphtheria teaches the same lesson.

How far this coincidence of date between the legislative Act of 1870 and the change in the age incidence of diphtheria mortality is more than mere coincidence it is impossible on these data to assert, but whatever is the cause of this change it is not related to London only, for the figures of England and Wales betoken the same change; and, further, if we group the more populous districts together, and the less populous districts together, we find the mortality statistics of both groups indicating the same change. Dr. Longstaff has shown that diphtheria in the three periods, 1855-60, 1861-70, and 1871-80, has not shown the same tendency to cause mortality in densely inhabited, as in sparsely inhabited districts, but that the much greater tendency of this disease to cause mortality in sparsely inhabited districts (which is shown by the figures of 1855-60) is less manifested in the period 1861-70, and very much less in the period 1871-80. The incidence has, indeed, been shifted from the less populous to the more populous districts. Is this result due to the same cause as that which has led to increased incidence upon the ages from three to ten years? Are we to assume that the difference between the amount of aggregation in schools of children living in the more densely inhabited districts in 1871-80 as compared with 1855-60 and 1861-70, has been greater than the difference between

the amount of aggregation of children in rural districts in 1871-80, compared with 1855-60 and 1861-70, or, in other words, has the Act of 1870 made a greater difference in the conditions of densely inhabited than in sparsely inhabited districts? It has been suggested to me that this may have occurred, because under the conditions of village life the more direct contact between the clergy and the parishioners would before 1870 have secured for the latter a larger use of schools than was possible in the case of persons living in large towns. If this be the case, a compulsory law would probably make more change in the towns than in the country districts. I do not think the grounds exist for more than speculation, but I may add that if such hypothesis be correct, we ought to find but little, if any, further shifting of the incidence of the disease in these communities between 1871-80 and 1881-90, when the facts as to the latter decennium are known. The great change would have occurred between 1861-70 and 1871-80, and probably not be repeated.

We might, however, expect the influence of schools, so far as they may give opportunity for the communication of infectious disease from person to person, to be manifested in another way. It might be expected that any interruption in school-life would be indicated by a diminution in the amount of prevalence. The most complete examination into this subject with which I am acquainted is that of Dr. Arthur Whitelegge, who, by recording the date of onset of cases of scarlet fever at Nottingham, found that fewer cases occurred on Wednesday, a day corresponding with diminished infection on Sunday. Even when the dates of onset are unknown, and only the dates of notification are available for our information, we might hope to obtain some evidence of the influence of longer holidays when the figures are sufficiently large to give trustworthy indications. I am fortunate in having for my purpose the record of notifications of infectious diseases for the vast population of London, and I am thus able to exhibit curves after the method of Buchan and Mitchell for each of the diseases, scarlet fever and diphtheria, for the years 1892 and 1893, showing the excess and defect of the cases notified in each week in relation to the mean of the weekly numbers.* Interest, of course, centres in the question

* Diagrams showing the curves for the year 1893 and appended to this paper are reproduced from the Annual Report of the Medical Officer of Health of administrative County of London.


All Ages
% below mean
- 40
- 50
- 60
- 70
- 80
% above mean
+ 80
+ 70
+ 60
+ 50
+ 40
+ 30
+ 20
+ 10
Mean
% below mean
- 10
- 20
- 30
- 40
- 50
- 60
- 70
- 80

whether at the period of the summer holiday there is any evidence of diminished prevalence, and the curves to which I refer leave no doubt in my mind that this has definitely occurred.

At the risk of being tedious I will state the facts separately for each disease. In 1892 scarlet fever was increasing in London in the twenty-seventh, twenty-eighth, and twenty-ninth weeks; in the thirtieth week it was somewhat diminished. In the thirty-first week there was a marked fall, which was maintained in the thirty-second week; there was a slight rise in the thirty-third week and a fall again in the thirty-fourth week. In the thirty-fifth week a considerable rise occurred, after which the disease increased rapidly in successive weeks. The depression in the curve is, therefore, shown in the thirtieth week, and conspicuously marked in the thirty-first, thirty-second, thirty-third, and thirty-fourth weeks. In this year, 1892, the London School Board schools generally closed at noon on Thursday, July 21st—*i.e.*, near the end of the twenty-ninth week. If we regard this closure as diminishing the opportunity for the spread of scarlet fever we should expect the result to show itself, perhaps, by slightly diminished prevalence in the next week, the thirtieth, and to be more conspicuous in the four subsequent weeks, allowing two or three days for the incubation of the disease, two or three days for the disease to become recognised, and perhaps a little delay in the notification. We should, moreover, expect an increase of prevalence the week after the schools reopened, and this is what actually occurred. Again, in 1893 scarlet fever fell in the thirty-second, thirty-third, thirty-fourth, and thirty-fifth weeks, and increased very considerably in the thirty-sixth. The holidays began on Friday, July 28th—*i.e.*, near the end of the thirtieth week. The thirty-first week showed a very slight increase of prevalence, and a very marked depression occurred in the thirty-second, thirty-third, thirty-fourth, and thirty-fifth weeks, a rapid rise occurring in the thirty-sixth week—that is, the week after that on which the schools reopened. It is noteworthy that the schools in 1893 closed and reopened a week later than in 1892, and the depressions and rise follow in each year in proper sequence. I have prepared a similar curve for the year 1894, and I find the same August depression, but inasmuch as the year is as yet incomplete, I prefer not to discuss it at the present time.

I have, however, another means of examining the curves

of these years. The closing of schools ought to be attended by some difference in the age incidence of the disease if it is associated with diminution in the amount of disease caught at school. If the cases of scarlet fever notified are grouped into three age periods—under three years of age, above three years of age and under thirteen years of age, and above thirteen years of age—the children from three to thirteen years of age ought to have enjoyed a larger immunity during the holidays than those at the other age periods. The London figures for 1892 show that this has been the case. It will be more convenient if I group the several weeks to which I must refer into periods embracing four weeks before the depression in the curve is strongly manifested, the four weeks during which this depression is thus manifested, and the four subsequent weeks. In these three successive months the number of cases under three years of age was 257, 322, and 321; the number of cases above thirteen years of age was 423, 460, and 529. In these two age periods, therefore, the numbers have increased in each period, but those from three to thirteen years of age have conspicuously fallen in the second period; they were 1760, 1605, and then they rose to 2441. The depression in the curve is therefore entirely due to diminished prevalence of scarlet fever at the school ages. If we examine them week by week we find that the fall coincides in point of time with the closing, and the subsequent rise coincides in point of time with the reopening of the schools, due allowance being made for the incubation period and delay in notification. In 1893 the results are very much the same. In the several four weeks antecedent to, during, and subsequent to the depression, the numbers under three years of age were 389, 386, and 422; the numbers over thirteen years of age were 803, 667, and 843; while the numbers from three to thirteen years of age were 2653, 1990, and 3269. There was, therefore, some fall in the number of cases at each age period; but whereas the infant fall was 1 per cent. and the fall of persons over thirteen years was 17 per cent., children at the school age fell 25 per cent.

I have already referred to diphtheria, and have pointed out a change in the age incidence of mortality from this disease which is suggestive of increased opportunity for the dissemination of diphtheria by means of schools. It is right, therefore, that the summer notification figures should be examined in the same way as in the case of scarlet fever. In 1892 it cannot be said there is an

August depression, the numbers in the three periods of four weeks being at all ages 639, 632, and 783. At the several ages the numbers were: under three years of age, 82, 98, and 91; thirteen years of age and upwards, 245, 221, and 296: and at the school age, 312, 303, and 391. In this year, therefore, the figures teach no definite lesson. It is, however, deserving of notice that the males and females behave very differently, and whereas the number of males at school age shows in the holiday time a fall of 16 per cent., against a fall of less than 4 per cent. among those over thirteen years of age, this special decrease of male children at school age is more than compensated for by a marked increase among the girls at school age in the holiday month. The year 1892 is therefore unsatisfactory. In the year 1893 there is a marked August holiday depression corresponding with the scarlet fever depression of that year. In the four week periods the number of cases were as follows: under three years of age, 158, 170, and 181; above thirteen years of age, 430, 433, and 577; while the numbers at school age were 556, 413, and 737. In this year, therefore, the whole of the holiday depression is due to diminished prevalence at the school-age period of life. I should be wanting in the caution that should characterise the student of epidemiology if I were to regard these figures as conclusive. I may claim for them that they are suggestive, that they harmonise well with the more exact observations of Dr. Whitelegge as to the behaviour of scarlet fever in Nottingham, to which I have referred, and that they certainly contribute to the reasons which already exist for further inquiry into the influence of schools in connexion with the prevalence of epidemic disease. They are, perhaps, especially deserving of consideration in this respect, because the August holiday occurs at a time when increase of prevalence of these diseases may be expected, and the fact that at such time there is an actual diminution of disease is the more striking.

It is of course possible that other influences, such as the migration of population from London, may have contributed to the August depression in our curves. To meet the requirements of the case we must assume that this migration was of very notable extent, and that children at school age migrated very much more largely than persons at other ages. I do not myself think that this explanation would suffice. Dr. Newsholme has, however, been good enough to supply me with the notification figures for the

two years 1892 and 1893 relating to Brighton. I have been glad to have them, because Brighton receives numerous visitors in the summer. Whether the exodus of children in boarding schools is in the holiday time compensated for by the influx of children who come as visitors cannot be stated, but Dr. Newsholme tells me that the number of cases of scarlet fever and diphtheria in boarding schools is so very trifling that they may be disregarded. The number of cases occurring in Brighton is very small, but the results of each of the two years may be stated. The year 1892 is wanting in the scarlet fever depression of August; in fact, there was in that year at all ages a slight increase in August. Of ages from three to thirteen there was a trifling fall in August, but the following four weeks the fall was more marked. Scarlet fever was therefore declining, and the numbers teach nothing. Of diphtheria the number of cases was too few to indicate anything except that there was a considerable rise among the children from three to thirteen years of age in the week after the holidays. In 1893, both in the case of scarlet fever and diphtheria, the results are very different. Scarlet fever was in the holiday weeks diminished by one-half, and more than doubled again in the post-holiday week, children from three to thirteen years of age being the chief gainers by the holiday. Diphtheria at all ages also fell in the holiday weeks, and rose again considerably in the following month. The whole of the fall was due to diminution of disease among children at school age. Again, I have gone further afield, and I am indebted to Professor Linroth of Stockholm for supplying me with the notification figures of scarlet fever for that city in 1893 and 1894. I am not able to distinguish between persons of different ages, but I can give the all-age figures; they have an interest, because the holidays begin in the middle of June and extend to about the third week in August. The holiday depression in 1893 is not specially conspicuous. The average number of cases per week in four weeks before the holidays, in the nine weeks of the holidays, and in the four subsequent weeks were respectively 36.2, 24.8, and 25. In 1894 the weekly average in five weeks before the holidays, eight weeks of the holidays, and five weeks after, respectively, were, however, 23.8, 13.1, and 41.6. This increase after the holidays, Professor Linroth writes to me, he has often observed. If we were to assume that in the particular years to which I have referred scarlet fever and diphtheria were disseminated in schools, it does

not necessarily follow that a similar behaviour of these diseases would be conspicuous in every year. It would be in agreement with the facts observed as to particular schools to find that at one time these maladies were spreading in schools and at another time that they were not doing so. Indeed, London diphtheria figures for 1892 themselves suggest that there was a difference in that year between the boys and girls. We may, however, learn something of the constancy of the August depression, whatever it may be due to, by reference to the death and case curves shown in the annual summary of the Registrar-General for 1890, the case curves being based on the cases admitted into the fever hospitals of London during sixteen years. We notice in the August deaths a slight interruption to the rise of the curve in that month, and at this time the case curve shows a marked depression. I have as yet said nothing of the influence of holidays at other times of the year, for the reason that my work is incomplete; but I may add that I have already suggestions that the same results will be found in connexion with them. On the other hand, Körösi, when dealing with the statistics in the manner I have described, states that the supposition that scarlet fever is a school disease is not borne out by the facts; "on the contrary, we repeatedly find the greatest number of cases occurring during the holidays"—a fact "which is five times the importance, seeing that this period is but a fifth part of the school time."

Before leaving this subject I desire briefly to refer to Körösi's conclusions as to the influence of schools on the prevalence of measles. He points out that in eleven years there occurred nine times, shortly after the beginning of the school year, either an epidemic or a marked rise in the number of cases, and again, at the close of the school year, measles has either greatly decreased or has altogether disappeared. At the same time he notes that the two coincidences—viz., that between attendance at school and epidemics of measles, and that between school holidays and decline of measles—have not the same importance, because the school year lasts from nine and a half to ten months and the holidays but from two to two months and a half. Since the school time is from four to five times as long as the holiday time, one single holiday epidemic is sufficient to counterbalance from four to five school year epidemics—that is, to weaken the argument that the school time and epidemics are associated as cause and

effect. He groups the number of attacks into weeks of school attendance and weeks of holidays, and shows a special incidence of the disease upon the former. But he is able to afford still further proof of considerable value. The summer school holiday until the school year 1876-77 was different from that of subsequent years, and he is thus able to show between these periods a shifting of the month of minimum mortality. Again, in two years of the latter period—1886 and 1892—the reopening of the schools after the summer holiday was postponed for a month on account of fear of cholera, and in these years the winter prevalence was correspondingly delayed. The difference of the date of the London scarlet fever depression in 1892 and 1893, which I have already pointed out, and the fact of postponement of the prevalence of measles in Budapest on occasions when the reopening of schools has been delayed, suggest that we may be able to obtain further evidence of the influence of schools on scarlet fever and diphtheria if we learn for other towns than London whether, in years in which the August depression occurs, this in point of time coincides with cessation and recommencement of school operations, for I believe the times of holidays are not identical in every English community. Probably the depression will not always be found. If, as seems probable, it is due to interruption of school operations, it would only be found when in the weeks preceding the holidays disease was spreading through the schools. This might occur in some years and again not in others.

I have already referred to other circumstances than social changes which affect the behaviour of the diseases under consideration. The condition of which I now wish to make mention is that of change in type of the disease. This subject has been so well discussed by Dr. Arthur Whitelegge in his Milroy Lectures for the year 1893* that I do not hesitate to refer freely to his work. He shows that the prevalences of scarlet fever occur rhythmically. There is, in fact, every four, five, or six years—sometimes more, sometimes less—a greater prevalence of scarlet fever than in the interval. These he calls minor waves. Beyond this he recognises major waves, extending over a number of years, and consisting of many minor waves. Midway between the greater prevalences which constitute the summits of the major wave the minor waves are longer and less distinct, but as they approach the next great prevalence indicated by the summit of the major

* *The Lancet*, Feb. 25th, and March 4th, 11th, and 18th, 1893.

wave they become shorter and more clearly defined. With this he observes corresponding changes in the fatality (case mortality) of the disease, an increased fatality in the maxima of the minor waves, and an especially increased fatality at the maxima of the major waves. We have, therefore, waves which we may call seasonal, waves extending over a few years, and waves extending over many years. But there is an important difference between the seasonal and other curves, for while Dr. Whitelegge finds an increased fatality in the years of the maxima of his minor and major waves, he failed to find evidence of this in the months of seasonal prevalence of scarlet fever. This difference between the seasonal and other waves demands a closer investigation. I have at the present time the monthly fatality in London for three years and for three diseases—viz., scarlet fever, diphtheria, and enteric fever. The figures are based on the cases notified each month, and the fatality is calculated on the deaths of a month beginning one week later. It cannot be a precise statement, but the method I have adopted reduces, I think, the error it contains as much as possible. Now, if we compare the fatality with the seasonal prevalence curves, we at once perceive that the seasons of high fatality of scarlet fever and diphtheria are not the same as the seasons of prevalence. Further, if we follow the fatality of these diseases month by month, we find that there is a general tendency for these diseases to have their highest fatality at the beginning of the year, and then for the fatality to decline until the month of August or September is reached, and then gradually to rise again, attaining a maximum in December. There is, indeed, an indication, and a strong indication, of seasonal fatality. The records of further years are wanting for more exact definition of the curve of fatality, but the statistics of the three years enable us to see roughly what it will be. Diphtheria is more uncertain in this respect than scarlet fever, but this may be due to the fact that with diphtheria we are dealing with fewer cases; so, again, enteric fever is still more uncertain, and perhaps for the same reason. We must be, however, I think, quite prepared to see enteric fever behaving differently from scarlet fever. Dr. Ballard has taught us that the fatality of diarrhœa increases with its prevalence, and in view of the saprophytic relations of enteric fever we may therefore find some disposition in the fatality of enteric fever to be governed by the same conditions as those which govern the fatality of diarrhœa.

This is a point upon which the material I possess gives no indications.

We may now consider some of the circumstances that may influence the fatality of disease, irrespectively of fatalities due to age and sex. It has been frequently observed that disease disseminated by milk and by water is attended by low fatality. Dr. Whitelegge's explanation that this may be due to the infection of persons of high resisting power is probably correct, for the contagium directly introduced into the body with food may perhaps cause a definite train of symptoms, though of mild degree, in those who would be able entirely to withstand contagium disseminated in other ways. Indeed, in outbreaks due to milk or water it is often difficult to draw the line between persons in disease and persons in health, so slight do the symptoms often appear. Thus, we find in households infected with enteric fever by a milk-supply severe and even fatal cases associated with others of less degree, even with persons who had no other indication of the disease than a diarrhœa lasting but a day or two. There is, however, reason for thinking that milk infection is, as a rule, less malignant in its results; certainly some of the milk outbreaks which have been investigated have been definitely characterised by the mildness of their symptoms, and I do not think the question whether some of the variation in the fatality of scarlet fever and diphtheria may not be due to varying proportions of that due to milk can be wholly set aside. It is not to be assumed that disease due to milk can always be demonstrated to be due to this cause. I have myself had experience of a severe outbreak of enteric fever in London in one year followed by a severe outbreak of the same disease the following year in a country town, and in both cases obviously due to the milk received from the same farm. But between the two outbreaks, in the winter months, the distribution of milk from this farm was attended by the occurrence of a very limited number of cases of the disease, so few, in fact, that but for the more serious outbreaks there would not be the least ground for associating them with the milk supply. More recently I have had experience of three cases of enteric fever occurring in a school of 150 boys, one or two cases of the same disease being subsequently found among a number of other persons receiving their milk from the same source. I can, therefore, well believe in cases due to milk infection being of far more frequent occurrence than can be demon-

strated, and affording, especially under the circumstances of town life, no evidence of the source of the infection. Beyond this we may recognise what Dr. Thorne Thorne has described as a "progressive development of the property of infectiveness". He has taught us how the beginnings of diphtheria are often indicated by prevalence of apparently simple sore-throat, and later how the disease is manifested by symptoms of a far more malignant malady.

From these circumstances alone we might expect variation in fatality if we assume that milk scarlet fever and milk diphtheria are more frequent at one season of the year, and that their contagia become more malignant after passing through the bodies of successive human beings. If, in fact, as suggested by Dr. Gresswell, the cow may play the part of an intermediate host, we may have in some degree to reckon with the varying conditions of the life of that animal. At the present time we cannot draw conclusions from the facts which have been observed. We may be rather disposed to think of seasonal fatality as related to those changes traced by Dr. Whitelegge over series of years, occurring rhythmically like those he has described and due to causes of which at present we have no knowledge. We can now only look upon the variations in the fatality of scarlet fever and diphtheria as "seasonal", leaving the future to determine what we must include under that term. For the explanation of the phenomena which present themselves to us we must find a condition or conditions which cause a fatality having its minimum in August or September, increasing in succeeding months to the end of the year, or into the following January, and then gradually declining, probably month by month, until the minimum is again reached. It may include meteorological conditions causing changes in the contagium after leaving one human body and before entering the next, or differences in the ability of the human body at the several seasons of the year to serve as a cultivating medium, and it may include differences in the original source of the contagium which is being disseminated. With such differences we shall have to take cognisance of those of prevalence, and probably also of symptoms; for Dr. Gresswell, in his thoughtful analysis of cases of scarlet fever in the South-Western Hospital, has described differences in the pulse-rate, in the gravity of the disease as exhibited by the condition of the tongue, and in the liability to suffer from albuminuria, determined ap-

parently by the months in which the patients received their infection.

In attempting to sum up my somewhat discursive remarks I am aware that much of the material at my disposal is only suggestive. I have referred to social conditions, and especially to that which we familiarly know as school influence, as possibly playing at times a larger part in determining prevalence than that with which we have hitherto credited it. Of the conditions determining prevalence of epidemic disease we cannot set aside those which are meteorological, but the exact relations between these prevalences and conditions are still very obscure. Generally, if long periods of time be compared, there is association of prevalence and increased fatality; but if these be studied by the facts of each of the last few years there is reason for thinking that the months of prevalence and high fatality are not the same. There is, indeed, as definite a seasonal fatality as there is a seasonal prevalence, but the two do not correspond in point of time. We are still in ignorance of the conditions which determine these variations in prevalence and fatality, but among them we may have to include differences in the source of the contagium.

Gentlemen, I am well aware that if for the purpose of giving illustration of the study of epidemiology I had desired to show the triumphs which have been attained I might have selected subject matter very different from that which I have discussed. Such, however, was not my intention. I preferred to utilise material which was nearer to my hand, and which demonstrated better the difficulties which attend us in our work. But notwithstanding these difficulties I am satisfied that we are—by adoption of design in our inquiries—gradually laying a sure foundation, that every contribution to our stock of knowledge, however small, helps to pave the way to a more clear understanding of the behaviour of epidemic disease. We work under more favourable conditions than the early members of the society; we have the knowledge they themselves have left us, we have the aid of the better records of notification of infectious disease, and we have the aid of a laboratory which was unknown in their time. To the last I have made no reference in my address, but its value and importance are recognised by us in the fact that at our next meeting this branch of our subject will be dealt with by Dr. Klein.

THE RELATION OF BACTERIA AND THEIR TOXINES.

By E. KLEIN, M.D., F.R.S., Lecturer on General Anatomy and Physiology in St. Bartholomew's Hospital.

(*Read: December 14th*, 1894.)

ONE of the best established propositions in bacteriology is, that bacteria when growing in the animal body or in artificial media produce changes which are of a definite and in most cases of a specific nature. This knowledge followed almost as a necessary result of Pasteur's work on yeast, bacterium aceti and micrococcus ureæ, which produce the alcoholic and acetic fermentation and the conversion of urea into ammonium carbonate respectively.

The time at my disposal does not permit of my entering into a review of the important researches of Selmi, and particularly of Brieger, as to the nature of the ptomaines and similar bodies . . . . the subject to which I would specially draw your attention this evening is the nature of the activities of pathogenic bacteria. For convenience I shall formulate two propositions, adducing facts in support of each, and then I shall go on to discuss certain considerations having to do with this subject matter.

PROPOSITION 1: *Pathogenic bacteria produce by their growth and multiplication specific poisonous substances which we will call toxines.*—These poisons differ one from another according to the species of pathogenic bacteria, and the changes induced in the animal body by different microbes are due to these toxines. Before the existence and nature of these substances were recognised it was held—as, for instance, in the case of anthrax—that the induced illness was due to mechanical action exerted by the bacillus on the body of the affected animal. As the anthrax bacillus is an organism which grows freely, and perhaps more conspicuously, in the presence of oxygen, it was thought that this bacillus consumed all the oxygen in the tissues of the infected animal and in this way deprived it of life. It was soon discovered, however, that this was not a sufficient explanation, for it was found that an animal

could be made fatally ill with anthrax and yet antecedent to its death by the disease the bacilli of anthrax could only in small numbers be detected in its blood. Hankin was the first to demonstrate that it is possible to procure by artificial culture of microbes in nutrient media certain toxic substances comparable to toxic albumoses. Fraenkel afforded a like demonstration, and Sidney Martin elucidated the subject to a much larger extent; but before toxines were isolated by their methods evidence existed tending to show that certain bacteria produced toxines. Thus, the microbes of typhoid fever and septicæmia were known, when grown in an albumen mixture, to contribute to such medium a poisonous quality. Roux and Yersin were, however, the first to demonstrate that the broth in which the diphtheria bacillus had been cultivated contained poison in a very concentrated form. A whole series of researches were next made by Kitasato and Brieger on the tetanus bacillus, with the result of demonstrating that injection into the animal body of the pure tetanus toxines obtained in artificial culture produced symptoms identical with those induced if the tetanus bacteria themselves were introduced into the experimental animal and allowed to grow and multiply therein. There can, therefore, be no question whatever as to the proposition that the pathogenic bacteria produce in the animal body toxic substances.

PROPOSITION 2: *Toxines, as far as they have been investigated, are definite chemical bodies.*—As a result of chemical analysis by Dr. Sidney Martin, it has been found that albumoses and alkaloids are present in the animal body in anthrax; and that in diphtheria there is present in the false membrane a body which behaves in every respect like a ferment, which ferment in turn acting on the tissues produces acid and alkaloidal bodies and albumoses. Similarly it has been found that, in artificial media, the bacilli of diphtheria and tetanus elaborate each a ferment, whereas under like conditions the bacilli of anthrax elaborate albumoses and an alkaloid. What then, it may be asked, is a given toxine? Is it the secretion of a bacterium, like the ferment of diphtheria, for instance; or is it of the nature of a proteid body elaborated by metabolism in the medium in which the bacterium is grown; or, again, is it part and parcel of the bacterial body itself? The poisonous substances, or toxines, produced in the animal body by the life processes of bacteria, mentioned above, must be carefully distinguished from the poisonous substance or substances present within the

protoplasm of the bacteria themselves. All observations tend to show that there is a definite distinction to be drawn between the poisons which may be present in the bacteria themselves and the poisonous substances liberated or elaborated by these organisms. When certain bacteria are introduced in large quantities into the peritoneal cavity of a rodent these bacteria themselves, without any of their metabolic products, are capable of producing symptoms of poisoning. For instance, if the growth of a given microbe scraped from the surface of a solid culture medium be distributed in some neutral fluid and then injected into an animal, it is the bodies of the bacteria themselves that are introduced thereto, not the toxines, which they have elaborated in the culture medium. And it has been found, as a matter of experiment, that such inoculations produce, in the case of several species of bacteria, a poisonous action quickly causing fatal peritonitis; in two or three hours the animal is ill, and within twenty-four hours the illness is fatal. The typhoid bacillus, the cholera bacillus, and several other microbes behave in this way. It matters not whether the bacteria are introduced alive or dead into the peritoneal cavity. If the bacteria are previously killed by exposure to a temperature of 70° C. their bodies produce the same poisonous action, though it must be added that the poisonous action is more pronounced if smaller quantities of the living bodies are used than when larger quantities of the dead bacteria are employed. Evidently, when the living bacteria are introduced in small quantities they go on multiplying and produce their toxines. The bacteria *per se* are in some cases, but not in all, poisonous. Thus, if the anthrax bacillus, or the diphtheria bacillus, or the fowl cholera bacillus, in a dead state, is introduced into the peritoneum of rodents no poisonous action results. The protoplasms of these microbes are not, like the protoplasm of the typhoid bacillus or of the vibrio of cholera, poisonous. Furthermore, as regards microbes whose protoplasms are poisonous, if a non-fatal dose—sufficient to make an animal ill, but not to kill it—is injected into the peritoneal cavity such an animal is protected from further protoplasmic poisoning by the same bacterium; but it is not protected against the toxines elaborated by the same species outside the animal body in artificial media. A very curious observation has been made with reference to these bacterial bodies. They probably contain more than one kind of poison. If, for instance, the enteric bacteria are killed by heat, and the precipitate extracted from their bodies by

water, it is possible to wash out a substance which produces a febrile rise of temperature in experimental animals, and substances such as this are called "pyrotoxins" because they are capable of producing fever. These, however, are not the substances which have a permanent protective action when bacteria alone are introduced into an animal; it is some other kind of substance bound up in the protoplasm of the microbe that protects the animal against further inoculation with the bacterium *per se*.

And next as to antitoxins. When an animal has passed through one attack of a given infectious disease—that is, when an animal has served as a host for the growth and multiplication of a particular pathogenic bacterium—it is found that its blood has acquired a peculiar faculty. The blood has become capable, not only of inhibiting further growth of the bacterium, but also of neutralising the toxines produced by the bacterium (Behring). If the cholera vibrio is introduced into the peritoneal cavity of a rodent in quantity just sufficient to produce an illness that does not prove fatal, such rodent ultimately withstands repeated injections of otherwise fatal doses of the vibrio. And further, if the blood of the animal that has thus acquired immunity is taken from it and introduced into the peritoneal cavity of another rodent this second animal on injection with the vibrio will not become ill at all; it is protected (Pfeiffer). This has been shown in the case of tetanus, diphtheria, and some other infectious diseases. Animals, therefore, which have acquired immunity possess in their blood a something that they had not before, and this something belongs to a group of substances called "antitoxins". These antitoxins must be very complex bodies because they are capable, not only of inhibiting the growth and multiplication of bacteria, but also of neutralising the toxines previously elaborated by the bacteria, and these two functions, *i.e.* (*a*) protective or immunising, and (*b*) antitoxic action are, be it observed, utterly different.

How are these complex anti-bodies thus possessing dual functions produced? There is a variety of theories as to this. In the first place there is the theory that the antitoxins are produced by the tissues themselves as a sort of defence against the toxines which have gained access to them, the toxines being thought of as stimulating the tissues to the production of certain defensive substances which are therefore called "antitoxins". In diphtheria the antitoxins produced in this way by the cells of the tissues are considered to comport themselves just like fer-

ments. This is the theory held by the French school. The Munich school says: There is no reason why the toxines themselves should not become converted into antitoxins. There is, however, little evidence in support of this view; and, indeed, there is some evidence which militates against it. Moreover, it seems to be reasonable to assume that the toxines stimulate the animal body to the production of antitoxin. There is yet another theory, and that is that possibly the bacterial bodies themselves play a part in the production of the immunising or protective substances. This raises the question, What becomes of the bacteria themselves in the animal that has been made ill by them and has recovered? The theory that bacteria have been in such cases killed and swallowed up by phagocytes is long since dead and buried. It is now perfectly well established that acquired immunity is chiefly of a chemical nature, that is to say is due to the presence in the blood of anti-bodies elaborated during the previous attack, and therefore the theory of phagocytes has become superfluous. But the bacteria may nevertheless themselves yield some substance which may assist in the production of these immunising bodies. For when an animal is recovering from an infectious disease the bacteria which caused it degenerate *pari passu* with the progress of recovery, and their substances become absorbed and pass into the general tissues of the animal, so that the bacterial bodies themselves become part of its tissue juices. In some cases there is an excretion of bacteria, but not very generally; in most cases they are absorbed in the way I have noted. That incorporation of the dead bacteria with the tissues of the recovered animal may have to do with its own immunity and with the immunising quality of blood serum withdrawn from it is suggested by the following facts. When *dead* bacteria are introduced into the peritoneal cavity of a guinea-pig they are capable of protecting that animal's peritoneum against the growth and multiplication there of the living bacteria subsequently introduced. This is the case with the cholera vibrios, the typhoid bacillus, and the colon bacillus. Indeed, it is not very difficult to imagine that other substances superadded to the tissues—as, for instance, dead bacterial protoplasm —may form part of these antitoxins, and I, for my part, incline to this view. In a word, I suspect that antitoxic serum obtains from dead bacterial protoplasm that it has assimilated its immunising power of inhibiting the processes of living bacteria, and that its power of neutralising

the already formed metabolic products of bacteria is due to tissue change resulting from contact of the tissues themselves with metabolic poison. But, whatever the nature of these antitoxins, it will no doubt be found that they are not of the simple character that has been assumed—namely, merely secretions of tissue juices—they must needs be of a very much more complex nature.

EPIDEMIC DISEASE IN THE ROYAL NAVY.

By Fleet-Surgeon THEODORE J. PRESTON, R.N.

(*Read*: 18*th January*, 1895.)

When the Secretary of this Society honoured me by requesting that I should read a paper before the members, I felt much at a loss for a suitable subject. The peculiar conditions of a naval life afford but few opportunities for original research, so that in default of better material I beg to offer for your consideration this evening a few facts relative to the history of epidemic disease in the Royal Navy. Soon after commencing to gather together the materials from which this paper is in the main compiled, I found that I had attempted a task beyond my powers, that such a nosological history extended over a number of years and comprised very many forms of disease; it therefore became necessary to eliminate some of the less important maladies and to confine attention to those which have been especially prominent and have had serious consequences in temporarily diminishing the offensive and defensive powers of the ships manned by those who have become victims to their epidemicity.

A general consideration of the subject confronts us with three distinct varieties of epidemic diseases, viz., (1) those which have entirely disappeared from the medical returns of the Navy; (2) those which at this present date only occasionally appear; and (3) those which still remain to recrudesce at intervals, and which it is believed may, by stricter attention to the laws of health, to isolation, and to disinfection, be practically stamped out.

The examples which it is proposed to consider are, of the first type, scurvy and plague; of the second, yellow fever and cholera; and of the third, influenza, the eruptive diseases—especially small-pox—and epidemic pneumonia.

But prior to considering the question of these diseases in detail it is desirable to present to you some outline of the moral conditions of the Service a century since, and to enumerate the principal causes which have brought about

radical changes in the victualling and daily life of the seaman; food, clothing and exercise being potent factors in the introduction of, or avoidance of, epidemic disease.

Previously to the year 1825, half-a-pint of raw spirits—when spirits were issued in lieu of beer—was allowed to every person serving on board His Majesty's ships without reference to age or necessity. A salutary change was then introduced by the reduction of the spirit ration to four ounces, daily, and an allowance of tea or of coffee was granted in lieu of the remaining four ounces. When this change was introduced it was feared that the men would resist it and become disaffected; but this was far from being the case, they appeared rather to welcome an opportunity of ridding themselves of what had hitherto been an active cause of disease, and readily embraced a sure method of acquiring and encouraging habits of temperance. To give boys of eighteen half-a-pint of spirits daily was practically teaching them to become drunkards; they must have been always more or less under the influence of alcohol, and in their persons many diseases were excited by its influence, and all were exaggerated. Under the able administration of successive Boards of Admiralty this spirit ration has gradually been reduced until at this present date no pure spirit is issued as a ration to any person serving in H.M. Navy. Diluted spirits may be issued to men over twenty years of age, but only when diluted with three times its quantity of water; all sale, loan, transfer, or barter of spirit or other intoxicating drink is also absolutely prohibited. The result of such legislation has been to give to the Navy at this date a power of resistance to disease which it did not possess during the Napoleonic wars. It must also be admitted that the abundant and excellent provisions now supplied to ships of war contribute largely to the high standard of general health enjoyed during the past thirty years by the Royal Navy. Formerly, a man of war was for many reasons an object of aversion, epidemic disease in many forms being one. Scurvy, sloughing ulcer, malignant dysentery, small-pox, and typhus, often decimated ships' companies. Many causes, no doubt, combined to give rise to those maladies, but in the production of the first and most frequent, the fertile and constant source was insufficient nutrition, resulting from scanty and unwholesome food. Looking back at a distance of a century it seems to us a matter of surprise that such disastrous results were permitted to continue for so long a period. Until the year 1796 scurvy continued

to cripple our ships, and in many instances produced a large amount of mortality.

In 1797, the victualling of the Navy underwent radical changes, the quality of the provisions supplied was greatly improved and, consequently, the health of seamen. Scurvy, typhus, dysentery, and sloughing ulcer, which up to the period of change had produced great havoc, became comparatively rare and light in impression. Since this latter year still further improvements have been introduced into the food supply of the Navy, such as giving cocoa or chocolate instead of water-gruel for breakfast, and the substitution of tea for the afternoon allowance of spirits, with the general result that the four forms of disease above mentioned have entirely ceased to be epidemic, and are scarcely known except by name. Of the many improvements which have taken place within the last 200 years in the physical and social condition of the people, no one is to be compared with that effected in the health of seamen in the Naval Service, because none approach it in magnitude and importance. Within that period they suffered immeasurably more from disease than did the civil population, or even their brothers-in-arms of the army; now they will not lose in comparison with the former and even with the extra risk inseparable from prolonged service in the tropics, compare favourably with the latter.

Palatable water in sufficient quantity is essential to comfort and has a marked influence on health, and in no article, at least in the manner of obtaining and storing it, has there been greater improvement than in this indispensable one in recent times in ships of war. When water was kept in casks it became slightly fœtid in a few days, especially when this fluid was impure and the temperature of the surrounding atmosphere high. When the solid food at sea consisted of very salt beef and pork, hard long-baked biscuits and puddings made of salt meat and flour, the desire for abundance of water was great. All those evils and inconveniences were banished from the Navy in the year 1815 by the substitution of iron tanks for water casks, further improvements being made by the careful selection of water from the storage reservoirs or mains of private companies on shore, and by the introduction of distillation from sea-water on board.

As far back as the year 1636 records exist that scurvy was known in the Navy; the causes of its appearance were

fairly well conjectured and the remedy for its prevention known. The attention of those entrusted with the victualling of the Navy was as early as this date drawn to the antiscorbutic virtues of lemon-juice, principally by John Woodall, Surgeon to St. Bartholomew's, who in 1617 published his "Surgeon's Mate, or Military and Domestic Medicine"; but despite the writings of Brucaei in 1690, Eugaleus in 1724, Lind in 1757, followed by Thomson, Trotter, Patterson, and Milman, no effort was made to check the disease, its presence being considered one of the usual occurrences inseparable from a seaman's life. When, in 1726, Admiral Hozier sailed from England for the West Indies with seven ships of the line and twice lost his whole crew by scurvy, followed some few years after by Lord Anson losing from similar causes four out of five men of the original crews with which he sailed from home, public attention was aroused to the extent that it was necessary to do something to check the alarming mortality from this malady. But the consideration of the question appears to have been postponed until 1795, when the safety of Lord Howe's Channel Fleet having been seriously endangered from the decimation of the crews by scurvy, the Admiralty were forced into supplying men-of-war with lemon-juice, preserved meats, and purer water. The results amply justified the extra expense incurred, and the fleet were able to keep the sea for months together without returning to port to fill up vacancies caused by death. There can be but little doubt that had not the remedial measures suggested by the medical advisers of the Admiralty been carried out to their fullest extent the pages of history would never have told of St. Vincent, the Nile, and Trafalgar. These facts are amply corroborated by statistics, for in 1780 there were admitted into Haslar Hospital alone, 145 cases of scurvy; after the hygienic and dietetic improvements of 1795, the entries for this disease steadily diminished, until, in 1806, there was but one case admitted into hospital from the whole British fleet.

The disease was, however, destined to re-appear, for in the year 1838 there occurred in H.M.S. *Winchester* some twenty cases of what was described as "night-blindness", —this ship was then at sea between Trincomalee and the Cape of Good Hope—and ten further cases between the Cape and England. The disease is stated to have commenced after exposure to the damp and warm atmosphere of the lower latitudes and in crossing the line, and it

appears to have been kept up afterwards by the bright sunshine that followed that state of the weather.

During the time the ship lay in Simon's Bay—being then on her way home—namely, from the 19th to the 31st March, the whole of the sick recovered and were discharged to duty; but after leaving the Cape and while crossing the tropics the malady again made its appearance amongst the men who had previously suffered and continued to remain until the vessel arrived at Spithead. This disease is now known to have been one of the early symptoms of scurvy, the men had been imperfectly fed on salt provisions, with a limited supply of fresh water while at sea; but when at the Cape, where they were able to obtain fresh meat and vegetables, the whole of the cases rapidly recovered and remained in health until resuming sea rations.

Fifty-five cases of scurvy occurred in the following year in the ship's company of the *Alligator*, who, after a tedious voyage from England, were disembarked at Port Essington, on the northern shores of Australia, for the purpose of forming a settlement. The men were all young and healthy, having been selected for this particular kind of service. Their duties at first were of a most laborious nature, as the ground had to be cleared of trees and wells dug for a supply of water; dwellings had then to be erected both for shelter and for the reception of stores. Meanwhile, as the surrounding country, which from recent rains abounded in lagoons and swamps, afforded neither animal nor vegetable food, they were compelled to use the provisions brought with them as sea-rations from England; consequently, about the end of the fourth month of their residence on shore, symptoms of scurvy began to make their appearance; but for some time they were held in check by the opportune arrival of a small vessel laden with bullocks and pigs, which were purchased and killed for the use of the people; still the scorbutic diathesis was not removed. Towards the close of the month of April the want of fresh vegetables and fresh meat became marked, many of the crew suffering from pain, swelling and discoloration of the lower extremities, ophthalmia, anæmia, and spongy gums. As neither fruits nor vegetables nor fresh animal food could be procured in the settlement, nor in the neighbouring country, and as the disease was evidently gaining ground, on the 3rd of June the vessel sailed for Sydney. On that day there were twelve men on the sick-list with scurvy, but before she reached port on July 9th the number had increased to forty-two. During the

passage preserved meat and vegetables in small quantities were issued to those men presenting the worst symptoms, and to the whole crew an extra allowance of lemon-juice and sugar was also made; but these measures in no way arrested the progress of the disease. On arrival at Sydney, the worst cases having been sent to hospital, an abundant supply of fresh meat, fruit, and vegetables was procured for the rest of the crew. In the course of a few days the improvement in their general health was most remarkable, and in a month the disease had entirely disappeared. It was in consequence of the unproductive nature of the country that the settlement at Port Essington was finally abandoned in 1845.

In 1854, amongst the fleet then in the Black Sea, scurvy made its appearance after the seamen, especially those serving in the larger ships, had been two months on salt provisions. There were altogether 139 cases, of which two were fatal. The disease continued epidemic during the following year until the autumn, when, owing to a more liberal supply of fresh vegetables, and particularly fruit in the shape of oranges, the disease subsided.

No further cases of true scurvy appear in the Returns until the year 1870, when four cases occurred in the persons of Kroomen, natives of West Africa, temporarily employed in the *Nymphe*. These people in their native state are for the most part vegetarians and, consequently, being less able to withstand the deprivation of fresh vegetables, inevitable at sea, succumb to this disease sooner than the rest of the ship's company, notwithstanding the issue of lemon-juice and sugar. They entirely recovered after a supply of fresh vegetables was obtained. Similarly three cases of scurvy out of a total force of 47,460 men occurred the following year, also amongst Kroomen. These cases were traced to the custom of these natives not taking fresh potatoes to sea with them, as did the rest of the ship's company.

The last serious outbreak of this malady took place in the year 1876, not actually in the Royal Navy, but its occurrence should be mentioned as its victims were, for the most part, men selected for the special service from the Navy. I allude to the Arctic Expedition.

This consisted of 122 officers and men in two ships, and amongst them there occurred fifty-nine cases of scurvy, with four deaths. The disease appeared early in the sledging expeditions after the men had been confined to the ships for a considerable period without sunlight,

and breathing, for the greater part of each twenty-four hours, an impure atmosphere.

A most exhaustive report upon the cause of this outbreak was presented to both Houses of Parliament, from which it appears that the Committee of Enquiry were of opinion that the outbreak was due to two causes; firstly, the absence of lime-juice from the sledging dietary; and, secondly, the deprivation of fresh meat. The men who remained in the ships, and to whom specially preserved vegetables, fruits, potatoes and lime-juice were issued, escaped the disease entirely.

Plague is the second disease that has ceased for many years to appear in the Navy Health Returns. The Indian variety of the malady, which seems to be very nearly allied to typhus, has never made its appearance in our men-of-war. The Egyptian bubonic plague is known to have reached Gibraltar in 1649, and Malta several times, notably in 1813. The first medically recorded cases in the Royal Navy occurred in the year 1840, when two persons, a man and a boy, are stated to have died from "plague" on board H.M.S. *Acheron* in Malta harbour; they were the only members of the ship's company so attacked, and it was believed that the disease was communicated to them by a bear which had been bought at Alexandria, a week previous. The animal was in a filthy condition when received, and having broken adrift a few days afterwards it was killed and skinned by these two persons. The boy, it may be remarked, had visited the shore; the man had not left the ship. Part of the carcase was salted down and eaten; the disease did not attack any other individual of the ship's company, and the report concludes with the statement that the symptoms were unequivocally those of plague. It is, however, more than probable that the malady to which these persons succumbed was anthrax, a disease at this period almost unknown.

The following year, however, true plague made its appearance in one ship on the Mediterranean Station under the following circumstances:—In December 1840, H.M.S. *Zebra* was driven on shore in the Bay of Acre, near the town of Kaiffa. The officers and ship's company in the first instance took up their abode in tents pitched on the sand by the seashore; here they remained five or six weeks and enjoyed good health under the circumstances; subsequently they removed into Kaiffa, a badly-drained and extremely dirty town; they occupied a house of two stories high, the men being quartered on the ground floor,

and the officers in the floor above. A case of fever occurred here which proved fatal. About the time they left the tents a party of eighteen men were sent to Acre to assist in repairing the walls of that fortress, between whom and the ship's company at Kaiffa there was frequent communication. On the 12th February, in consequence of several cases of plague having occurred in Acre, the men were removed to the quarters at Kaiffa.

On the 17th the *Castor* arrived off Kaiffa to render assistance to the wrecked vessel. A party of men were landed for the performance of these duties, and were domiciled in the house occupied by the *Zebra's* men, an equal number of the latter being placed on board the *Castor*. Thus matters remained until the evening of the 20th, when one of the latter party was attacked with the disease, which proved fatal in forty-eight hours. Nine other cases soon followed amongst the men on shore, when orders were received to re-embark all persons and proceed to Malta. The true character of the disease appears not to have been suspected until the 23rd inst., on which day two of the patients complained of swellings in the groins and armpits. This led to a general examination of the sick on these points, when it was ascertained that the greater number were so affected. On the 25th, two other men were attacked and one on the 27th inst., the total number of unquestionable cases of plague was therefore thirteen; ten of these occurred amongst the men belonging to the *Zebra* who had been at Acre, and one amongst the party of artificers landed from the *Castor;* of the whole number, nine cases terminated fatally. The surgeon of one of the ships considered that the "agency of contagion in the origin of the disease could not be denied"; the other, from the number of cases which occurred amongst the men who had worked at Acre, where it was known the disease then existed as well as in some of the adjacent villages, merely states that these circumstances afford presumptive proof that it was imported from Acre.

There is one fact, however, which forms a singular contrast with these views, namely, that it was not communicated to any of the *Castor's* men during the voyage to Malta; moreover, its character not having been suspected until the 23rd segregation on board was not enforced. Segregation, indeed, in a ship of war, particularly in one with a large addition to her complement, is next to an impossibility. Certain men attended upon the sick, but neither they nor the medical officers contracted the disease;

the nurses were purposely kept in ignorance of the true nature of the malady.

The *Castor* arrived at Malta on the 9th of March, and the ship having been placed in quarantine, the remaining cases rapidly recovered. It may here be mentioned that some dozen medical officers and seamen of the Royal Navy have recently been employed by the Government of Hong Kong during the epidemic of bubonic plague in that island. Not one of them contracted the disease or appear to have suffered in any way from their contact with the sick.

Yellow fever has been known to naval medical men for upwards of two centuries, and it was believed that the poison might originate on board ship, independently of any communication with an infecting town. La Roche states that many instances have occurred of an outbreak of this disease where no communication had existed, as the vessels had not approached the land within many miles. They had sailed from Europe, not perhaps in the best condition of stowage or of cleanliness, probably undermanned, and with a dubious quality of drinking water on board, and yellow fever appeared as the ships experienced the high temperature of the tropics. But time will not permit of the consideration of the earlier history of the disease, suffice it to detail an account of the introduction of the malady into the island of Ascension.

Early in the year 1837 yellow fever, which had not been seen there for seven years, broke out in a vessel lying off Sierra Leoné, extended to the merchant shipping, and in May appeared in H.M.S. *Curlew*. Of the extent of this epidemic no record exists, as the medical officer was one of the first victims, but at least one half of the ship's company died, including a second surgeon. When the disease manifested itself the ship was sent northward to the Gambia, where after arrival the cases were removed, as they occurred, to sick quarters on shore. As this fever broke out amongst the white population at the Gambia shortly after the sick of the *Curlew* were landed, it was reasonably enough supposed to have been introduced by the latter, and this is the more probable as the disease had not within the recollection of the oldest inhabitant been known in that part of the country, and unquestionably it has not since made its appearance. This fever continued to remain in evidence in Sierra Leone and its vicinity during the summer and autumn of the year, when on November 30th, the *Ætna* anchored off the town. None of the crew landed, but some of the officers and stewards did do so. After re-

maining three days she went to sea. Seven days later yellow fever made its appearance, and extended for fifty days, attacking in succession every individual in the ship, excepting five men. The vessel arrived at Ascension towards the end of January and landed the sick, placing them under tents as a temporary hospital. Two other vessels, the *Forester* and the *Bonetta*, which had also contracted yellow fever at Sierra Leone, arrived about the same time and also landed their sick. These ships had already lost by disease their medical officers and the greater part of their crews, and had almost drifted to the island. Quarantine was supposed to have been established between the temporary hospital and the remainder of the island, but little precautions to prevent the spread of the disease appear to have been taken. Foul linen was also landed from the ships to be washed, and deceased officers' effects, including articles of personal clothing, which had been previously well aired, were sold by public auction to residents on the island. Two or three weeks after, the disease broke out suddenly amongst the garrison and rapidly spread. After about three months it entirely ceased, and from that time to the present it has not reappeared. The memory of the fatality of this epidemic of over half-a-century since remains so strongly impressed that no ship with yellow fever on board is now likely to obtain permission to have the slightest communication with the island. It may be remarked that Ascension is governed as is a man-of-war, and is a tender to the stationary flag-ship at the Cape.

Passing by minor epidemics, which occurred at intervals in the West Indies and South America, we reach the year 1864, when a serious outbreak of the disease took place and lasted for several years. In the summer of this year, yellow fever prevailed at Nassau, in the Bahamas, from whence it was carried to Bermuda by a passenger conveyed in a mail steamer. With the knowledge that yellow fever was prevalent at St. George's, and that it would probably follow its usual route, the naval authorities had ample time for the adoption of hygienic measures in the guard-ship and dockyard establishments. Every part of the ship was thoroughly cleaned, the bilges washed out with chloride of zinc solution and well ventilated. The men were encouraged to thoroughly wash themselves, and their bedding was frequently aired. But all was to no purpose, the fever appeared on board; hygienic precautions, extra food and rest at mid-day, had no protective power when once the

disease had become thoroughly epidemic in the vessel. Eighty persons were attacked with the disease, of whom one half died.

Further epidemics of this malady took place during the following three years in the West Indian islands, particularly Jamaica; the West Coast of Africa, into which it had been reintroduced by a slaver from the island of St. Thomas; and at Panama, also infected by a passenger steamer from St. Thomas'. During the five years, 1863 to 1867 inclusive, the Naval Service lost no less than 142 men from yellow fever alone.

In 1869 an extensive outbreak of this deadly form of fever occurred at Port Royal, Jamaica. The disease had been more or less in evidence in the whole island during the preceding five years, but its activity and powers of attack appear to have been remarkably augmented by the introduction of a powerful dose of the poison from the neighbouring island of Cuba. The *Barracouta*, an English man-of-war, remained at Havanna twelve days. The Spanish health authorities declared the town free from disease, and it was only by an accident that the medical officer of the ship found out that yellow fever was epidemic in the town, and had appeared amongst the merchant shipping. British interests necessitated the stay of the *Barracouta* until 1st July, but every precaution was taken that the ship had no communication with others, and that no person landed except when absolutely necessary. She left Havanna in perfect health, and five days later arrived at Port Royal. On the 11th July, yellow fever appeared in the ship. She was at once sent north to Bermuda, whence, as other cases still occurred, she was despatched to Halifax. The *Eclipse*, *Defence*, *Philomel*, and *Jason*, also acquired the disease in a similar manner at Havanna, and so fatal were the results that they called forth a protest of indignation from all European medical men in practice in the West Indies, that the concealment of epidemic diseases in foreign ports by the local authorities was culpable in the extreme, and they suggested that consular officials should signal and warn men-of-war approaching any harbour wherein yellow fever had made its appearance. In this epidemic alone the Navy lost fifty-seven men.

During the three following years the disease did not assume an epidemic form in the service, but in 1873 an extensive wave of the pestilence spread over the West Indian islands, Central America, and the north-east, east, and western coasts of South America. In this year, H.M.S.

Duncan arrived at Port Royal, Jamaica, from England, in a crowded condition, and with a new crew for the *Aboukir*, stationary guard-ship at that port. The men were granted leave, and spent most of the time in Kingston, where yellow fever may be said to be endemic. Six days after, one of the newly arrived crew was attacked with the disease, which was recognised when it was too late, the malady became epidemic on board, and through the medium of men employed in the dockyard but living on board the ship, the whole of the naval establishments on shore were infected. The disease was of a very malignant type, and in conformity with former local experience proved most violent and fatal in the healthiest men.

At Rio de Janiero the new year was ushered in with an epidemic of yellow fever appearing amongst the merchant shipping, and following upon an epidemic of small-pox on shore, which was then declining. The disease was unfortunately communicated to H.M.S. *Egmont*, store-ship at that port, by one of her own men who had been exposed to the contagion. Twenty-eight cases of the disease occurred, but only two were fatal. Local disinfection and quarantine appear to have been practised in this ship with excellent results, the medical officer remarking that cleanliness and ventilation were the best preventives against its spread. Other English men-of-war who visited the port were not infected.

The last epidemic of yellow fever which calls for any particular notice occurred in 1882 amongst the men-of-war cruising in the vicinity of Port Royal. As on many previous occasions the outbreak was a recrudescence of the disease, which is undoubtedly endemic in Jamaica. On this occasion there were thirty-six cases, with seven deaths. Since this year the disease has manifested itself only in isolated instances, and no epidemic of the pestilence has been recorded. The malady may be considered as a preventable one; the experience of the past seven years proves this assumption. This period has been one of improvement in hygienic measures, in sanitary precautions, in the study of the causes and the prevention of the disease, and the results are shown as under:—

Period,	1863 to 1867.	Cases, 267.	Deaths, 142.
„	1868 „ 1872.	„ 178.	„ 77.
„	1873 „ 1877.	„ 106.	„ 49.
„	1878 „ 1882.	„ 54.	„ 21.
„	1883 „ 1887.	„ 47.	„ 14.
„	1888 „ 1892.	„ 9.	„ 5.
„	1893.	„ 1.	„ 1.

By the substitution of iron and steel vessels for those of wood, by the more perfect nutrition of the men and the exercise of the common sense of hygiene, it is to be believed that yellow fever will become as unknown in the Royal Navy as now is typhus.

The medical records of the Navy give little indication of the existence of cholera in epidemic form during the early part of this present century; but as many vessels appear to have suffered from outbreaks of dysentery it may be assumed that the disease has been present, but imperfectly recognised. The earliest outbreak of this disease of which there is any graphic account occurred in the year 1837 at Malta, following closely upon the decline of an epidemic of influenza. In this year cholera was prevalent in Italy and Sicily, but by the enforcement of rigid quarantine the disease had hitherto been kept out of the island. On June 8th, however, a Turkish man-of-war arrived at Valetta from Tunis, and was with other merchant vessels placed in quarantine. These ships were anchored head and stern in a line and also secured to the shore, the water being very deep in the quarantine harbour. One of the sanitary regulations of the port was to the effect that the crews of vessels undergoing quarantine should get the whole of their wearing apparel and bedding on deck, and expose them thoroughly to the sun and air. Immediately over where these ships lay—there were generally twenty-five or thirty of them—was an extensive poor-house then containing about 800 persons, chiefly old men and women. This building was situated low, in a kind of dry ditch, part of the fortifications constructed by the old Knights of Malta. Here the disease made its appearance on June 9th, on which day two cases occurred (both terminating fatally) in apartments nearest the Turkish frigate in which cases had presented themselves while she lay at Tunis. Numerous additional cases continued to occur, both in the ships and in the poor-house, until June 13th, when about 600 of the inmates, then apparently in good health, were removed to spacious barracks at the entrance of the Grand Harbour, in which were anchored the English men-of-war. Removal of these poor people was of no avail. The disease rapidly spread amongst them, and in a very brief space of time swept off 400 persons. From this building practically the whole island, and many of H.M. regiments then stationed there, were infected. The *Rodney* was lying in Malta harbour when the disease first made its appearance in the capital. She went to sea on the 12th June, and did not

return until the 29th. A supply of fresh water was taken on board from the naval tanks with all possible speed, and early in the morning of July 1st she again left the island. On the following day, at 7.30 a.m., the first case of cholera showed itself, and the patient died a few hours later. This man had neither been on shore nor absent on duty in any of the boats when the ship lay in Malta harbour. The disease extended rapidly while the vessel was at sea between the 2nd and the 13th of July, attacking ninety-four persons, of whom ten died.

The *Caledonia*, whose ship's company had suffered severely from influenza during May and June of this year, was reported, towards the close of the latter month, as being troubled with an epidemic of diarrhœa, in view of which she was ordered to cruise at sea, but in proximity to the island. Early in July she returned to the Grand Harbour, remaining for several days, and communicating with the shore. On the 7th July the first case of cholera appeared, which was fatal. The epidemic lasted until the 20th, declining with numerous cases of diarrhœa. Altogether in this vessel there were treated 100 cases of diarrhœa and sixty-two of cholera, with five deaths. The *Russell*, *Confiance*, *Asia*, and *Hermes* also suffered in a similar manner, the loss sustained by the whole squadron amounting to thirty-eight men out of a total of 219 cases. As all these ships were in the habit of sending their soiled linen to be washed in the vicinity of the poor-house mentioned as the focus of the spread of the infection in the island, there is every reason to believe that the disease was communicated to them by the native washerwomen. It should be mentioned that the naval water tanks were isolated and carefully guarded from risk of infection.

The occurrence and disastrous results of this epidemic is still remembered by the inhabitants, and there is no doubt but that the disease was true Asiatic cholera, which seems, however, to have visited the naval force comparatively mildly.

There is one circumstance respecting the non-communication of the disease which deserves to be recorded. During the whole time that it continued its desolating course over the island and in the shipping, although there were upwards of seventy cases introduced and treated in the wards of the naval hospital, it was not communicated to one of the individuals therein resident, whether patients, nurses. servants, or medical officers, these being of all classes, and a population of about 100 persons within the buildings, many

of whom, moreover, were in that state of health, mental and bodily, which predisposes to an attack: but although not actually seized with cholera, many of them suffered from diarrhœa subsequently to the admission of the cholera cases. This hospital stands high, is exposed to all winds, has an independent water supply, and its own laundry.

In June, 1854, cholera was introduced into the camp of the allied armies in the Crimea by means of an infected transport which had arrived from Marseilles, a port then, as now, a recognised focus of dissemination of this disease. The fleets remained free from the malady until a French steamer conveying troops arrived at the anchorage and was sent inshore of the English fleet, and placed in quarantine; at the same time her sick were landed and housed under canvas. The weather at the time was tempestuous, and the wind, blowing from the shore, passed first over the tents and the troop-ship and then across H.M.S. *Diamond*, bearing with it any emanations or effluvia which might escape from the sick or from their evacuations. Two days after, cholera occurred in this latter vessel and rapidly spread to the rest of the fleet in its most malignant form. In one ship alone—the *Britannia*—200 men were attacked in four days, of whom ninety-three died. She carried 920 officers and men, of whom 625 suffered from the disease, and 139 died—a serious loss to occur amongst a fighting force engaged in active operations against an enemy. The epidemic did not subside until the ship was emptied of her crew, fumigated, cleansed and repainted. Cholera was also introduced into the Baltic fleets by an English man-of-war, the *Hannibal*, which carried troops from France, port not stated, but believed to have been Brest. It subsequently extended to the force disembarked before Bombarsund, and to the Russian prisoners subsequent to their capture. Altogether there were 310 cases, with 108 deaths.

In 1862 the disease was communicated to the squadron in China. It had had its origin amongst the rebels in China, and had spread to the towns they were investing and from thence in a northerly direction to the Peiho. At Ningpo and Shanghai it committed great ravages amongst the native community, and spread to the merchant shipping and men-of-war. From the coast of China it was conveyed to Japan, but in no instance did the disease appear in any man-of-war excepting in those localities in which it had been prevailing epidemically. In the *Euryalus* there were twenty-one deaths in thirty-nine cases, all referable to the exposure to which the men were subjected in the expedition

to Khadung, a town then in the possession of the Taiping rebels, and the surrounding country, a hot-bed of cholera. It is stated with reference to this outbreak that in the military forces, who were equally exposed with the Naval Brigade, there was not even a case of diarrhœa; they had been supplied with good water from Shanghai which they carried with them. The total number of cases during this outbreak amounted to 141, of whom eighty-nine died.

The island of Malta was in 1865 infected by refugees from Alexandria, Egypt having acquired cholera through the medium of the pilgrim steamers from Arabian ports. The epidemic appears to have been virulent in its nature, and as far as the civil population were concerned entailed a mortality of 63 per cent. The lessons of the epidemic of 1837 seem to have been entirely forgotten by the local authorities who, beyond ineffectual quarantine regulations against merchant shipping, made no proper attempt to stamp out the disease. The Naval regulations as to personal and local hygiene were very successful, but three cases occurring amongst the men-of-war, all fatal. Beyond doubt these men acquired the disease on shore, and most probably at some of the low public houses then existing in the vicinity of the dockyard.

The year following, cholera was epidemic in the United Kingdom, and caused much mortality. Portsmouth appears to have been the naval port most severely visited by this malignant disease. In the Home Force, the prompt preventative measures taken with respect to it were attended with the best results. Only fifteen cases occurred, with a mortality of eight, and of these the majority contracted the disease at their own residences on shore, or while absent from their ships on leave. Both during the accession and the decline of the epidemic, diarrhœa was unusually prevalent amongst ships' companies, the number of cases of this disease totalling 1,227, in comparison with an average of 300 in former years.

An outbreak of cholera occurred in 1871 on board H.M.S. *Magpie*, then anchored some two miles off Bahrein, in the Persian Gulf. Cholera was endemic in that port and in the surrounding country. The surgeon of the ship was the only person who had any communication with the shore, he having landed to afford professional assistance to the Governor of the port. The day following he was taken ill with the disease and died. The disease was communicated to others who waited upon the deceased officer, but did not extend when his successor had arrived on board and com-

menced to isolate the cases as they occurred. Great advantages in limiting the number attacked appear to have resulted from compelling those in attendance upon the sick to take a hot water with soap bath daily.

The origin of the epidemic in the Persian Gulf was its conveyance by famine-stricken Persians flying from Teheran, Shiraz, and other towns of that country. Famine-fever had, in this instance, preceded the appearance of cholera. At this time the disease was epidemic in Bombay, and had attacked a native regiment, who on its occurrence had shifted their quarters to the beach immediately to windward of where H.M.S. *Teazer* lay at anchor. From this camp a seaman was infected and died, as also did the captain of a merchant steamer who was exposed to the same influence. No other cases occurred amongst the shipping, nor in the *Teazer* after leaving the infected locality.

A small gunboat, with a crew of 55 men, stationed at the mouth of the river Danube, lost eight men from cholera in 1873. The disease first appeared in the Turkish camp of exercise at Schumla, and spread rapidly into Roumania and Hungary by way of Widdin and Rustchuck. The *Cockatrice* lay at Turna Severin in Roumania, where the disease became endemic in a fatal form, with an average mortality of 56 per cent. Every precaution had been taken to prevent its introduction into the ship, but without success. The vessel went to sea, and no further cases occurred. There can be but little doubt that in this instance the disease was entirely wind-borne.

From this year, cholera in epidemic form can hardly be said to have appeared in the Service until 1891, although sporadic cases are returned in every year to that date. Calcutta, the Burmese and Chinese ports, are reported as being the *foci* whence the cases were severally infected. The mortality from this cause during those years was not large, in fact, during the past seven years it has declined considerably. The experience of past epidemics, and the researches upon its means of communication from man to man, have educated the Service into a knowledge of the methods of avoiding the disease or of minimising the chances of infection.

From the Table exhibited before you, it will be remarked that the year 1891 stands out very prominently, as fifty cases are reported to have occurred in that year. Of these, forty-four cases with twenty-three deaths were on the East Indies Station. Twenty-three cases occurred in the *Redbreast* and sixteen in the *Marathon*. The disease appeared

on board both these vessels on the 11th September, when they were at Bombay.

The first case was noticed in the *Redbreast* at 9 A.M. and in the *Marathon* at 3 P.M. on that day; the last case occurring in the former ship at 9 A.M. on the 15th September, and the final one in the latter at 7 P.M. on the 12th September. There were ten deaths from this disease in the *Redbreast* and nine in the *Marathon*. The *Redbreast* had arrived from Kurachee two days prior to the outbreak, while the *Marathon* had been at Bombay for thirteen days, her last port having been Trincomalee, but no cholera was known to exist at either of those places at the time of the departure of the respective ships; cholera was, however, present among the native population of Bombay at the period, although it had not assumed the proportions of an epidemic. At the time of the outbreak general leave had been given to the crew of the *Marathon;* one watch had already visited the shore and had returned to the ship, while the other watch was still on leave. Four men of this latter division were attacked with cholera at the Sailors' Home on shore, two being sent direct to hospital and two returning on board. Special leave had likewise been granted to some of the crew of the *Redbreast.* The disease was, therefore, in all probability primarily contracted by men while on leave at Bombay, and by them conveyed to the ships. The medical officer observes: "The two ships' companies were comparatively recent arrivals on this part of the station, and it is well known that new comers are especially liable to contract disease. Cholera is much more prevalent during some years than during others, and this year it is present to an unusual extent in various places on the Station, especially among Europeans." Five cases occurred in the *Blanche,* four being fatal. It appears that the disease was contracted at Madras. Cholera is a terrible malady to break out in a ship. There are no means of leaving the infected locality, while the depression produced by the mortality that ensues in the men's minds rather tends to increase the chances of their acquiring the disease than the reverse. It is no respecter of persons, in fact, the apparently strong are the first to be seized, while the chronic drunkard and bad character escape scathless. During the past twenty-three years the Navy has lost seventy-eight officers and men from this preventable scourge.

On May 2nd, 1782, Admiral Kempenfelt sailed from Spithead with a squadron, of which the *Goliath* was one. The crew of that vessel were attacked with influenza on

the 29th of May, and the remainder were, at different dates, affected. So many of the men were rendered incapable of duty by this prevailing sickness that the whole squadron was obliged to return to port about the second week in June. They had not had any communication with the land, but had cruised at sea solely, between Brest and the Lizard. In the beginning of the same month another large squadron sailed, all in perfect health, under Lord Howe's command, for the Dutch coast. Towards the end of the month, just at the time therefore when the *Goliath* became infected, it appeared in the *Rippon*, the *Princess Amelia*, and other ships of the last-mentioned fleet, although there had been no intercourse with the land.

The epidemic of influenza which occurred in 1837 was severely felt by the Royal Navy. The first case appeared on January 5th at Sheerness. It is not a little remarkable that the initial case of this malady in 1890 took place at this port just about the same relative date. During the latter part of this month the disease made its appearance amongst the merchant shipping lying off Lisbon. Early in February it appeared in H.M.S. *Russell*, 74, anchored in the Tagus. The first man attacked had been exposed the greater part of the day in a boat, and on shore. The disease then spread very rapidly throughout the ship's company. In the early part of March the disease was communicated to the *Rodney*, attacking 280 of her crew. The suddenness with which it fell upon them was very remarkable, as during the first two days from its irruption the seizures were at the rate of three men per hour. In May it reached Malta, where it attacked the ships' companies of most of the men-of-war then lying in the Grand Harbour. One ship, the *Volage*, sailed from the Bosphorus on May 3rd, arrived at Malta May 19th, and sailed again on the 23rd. On the 28th two cases occurred, thus making an incubation period of five days. Two days after another ship, the *Vanguard*, had left Malta harbour, influenza suddenly appeared, and in the course of forty-eight hours attacked upwards of 200 men, which compelled her to return to her anchorage: she had left the port with a clean bill of health. Every man-of-war that had been exposed to its infective power was smitten with the disease, to the number of upwards of three thousand persons. Towards the early part of the month of July influenza reached the island of Corfu, when it assailed the ship's company of the *Sapphire*, then at anchor there. It was generally supposed to have had its origin in atmospheric causes, the weather

when it broke out being described as cold, raw, and damp, but as the disease spread over the greater part of Europe, these local phenomena are of little importance in an etiological point of view.

The other cases of this malady which occurred in H.M. ships during this year were in the China station. The ship's company of the *Raleigh* were attacked in March while at sea between Singapore and Manilla, a very warm part of the world, and in the *Zebra* while at Penang. In both instances the epidemic was of an exceedingly mild nature.

On the Home Station, which then comprised only the British islands, out of a total force of 4,000 men there were only 976 cases of influenza serious enough to require placing upon the sick list, of whom two died.

As previously remarked, the epidemic commenced suddenly in the *Howe*, at Sheerness, on January 5th, the disease rapidly spreading up the River Medway until it appeared amongst the vessels lying off the dockyard at Chatham. On January 11th the guard-ship at Portsmouth was attacked, the next day it appeared in the *Victory*, and the day following in the *Excellent*. On January 16th the disease suddenly appeared amongst the ships at Plymouth. At all the naval ports the malady seems to have been severe at its onset, but died out in the course of two weeks. The official record of this outbreak concludes: "as there were only two deaths from this malady in a force of about 3,390 men, it is assumed that this low rate of mortality may be ascribed partly to their being generally free from organic disease, and partly to the care bestowed on the patients, not only during the attack, but subsequently, while labouring under its effects, it being the custom in the Naval Service not to permit men who have once been placed on the sick list to return to their duty until they are thoroughly recovered."

Here I regret to have to remark that a gap occurs in the records of the medical history of the Navy, for it appears that no collected information is available as to the epidemic of 1847, the health reports having ceased to be printed in the year 1843, and not resumed until 1854.

In the years 1854 and 1855, which for present purposes may be grouped together, epidemic catarrh made its appearance insidiously in the Naval forces assembled in the Crimea, the Black Sea, and the Baltic. The total forces numbered about 14,000 persons, amongst whom there occurred 5,618 cases of influenza in the ratio of 202 per 1000, with three deaths.

The records as to the incidence and decline of this disease are scanty, the medical officers' reports being largely composed of the then more important subject of surgery amongst those wounded in action.

In the year 1863 an isolated epidemic of influenza occurred on board the *Sutlej*. This ship, which was manned by an exceptionally fine, healthy body of men, left Vancouver's Island on September 24th for Valparaiso, calling at various ports *en route*. The sea-board towns of California and of Mexico were in turn visited: these ports were free from contagious and epidemic diseases, and the crew, being still in robust health, were but little affected by ordinary climatic influences. On November 14th, having then been five days at sea, and while crossing the Bay of Tehuantepec, at about 140 miles from the shore, a strong gale sprung up from off the land, attended by a remarkable phenomenon, and followed by an outbreak of influenza in so striking a manner that they could not fail to be regarded as cause and effect. Though a long distance from the shore at the onset of the gale, the wind reached the ship charged with the perfume of fragrant flowers, which scented the atmosphere for some considerable time. Two days after the gale two cases of influenza occurred in the ship, and the disease rapidly became epidemic. On the 18th of November the vessel reached La Union, a sea-port town of San Salvador, to find influenza prevalent in a severe form. It was from this coast that the perfume-laden wind had blown. The surgeon of the *Sutlej* remarked that the younger portion of the crew enjoyed a comparative immunity from the epidemic, very few persons under twenty years of age being amongst the sufferers.

The disease must have extended southwards along the coast of South America, but at a very slow rate, for it was not until August, 1866, that influenza appeared at Valparaiso. Here the stationary store-ship with a very small crew entirely escaped, but another man-of-war, the *Mutine*, suffered very severely. In this year, also, the *Topaze* contracted the disease at Callao, some 1,500 miles north of Valparaiso, and other ships found the disease prevalent in the Sandwich islands 6,000 miles distant from the coast of Central America.

An epidemic of influenza appeared in 1874 on board the *Adventure*, a troop-ship then on passage between England and the East. It continued to affect the passengers and crew from July 8th to August 25th, the ship being at sea in the South Atlantic Ocean, and having had no communication

with any port for a lengthened period. There can be but little doubt that in this instance, as in that of the *Sutlej* previously mentioned, the disease was carried on board by the wind.

No further outbreak of this malady appears to have taken place in the Royal Navy until the year 1889, when the so-called "Russian" influenza attacked the crew of the *Cockatrice*, a small gun-boat stationed at Kustendji, a port at one of the mouths of the Danube. The first case occurred on the 19th of December. The year following the disease became universal, appearing in the medical returns from every quarter of the globe, in greater or less degree of severity, and increasing in a marked degree the number of cases of disease and of days' sickness, a matter of serious import, representing as it does a vast loss of labour to the State—the increase of sickness in 1890 being represented by an augmentation, compared with the previous year, of 101·25 per 1000 of the total force. In the whole naval service afloat, that is to say, 53,350 officers and men, there were 7,821 cases of influenza, with six deaths. The past history of the epidemic was repeated in this year. The Channel Squadron arrived at Gibraltar on the 2nd of January; strong easterly winds blew, and on the 13th the first case of influenza appeared, to last for a month and affect one-third of the fighting force. The training ships, with their comparatively younger crews, suffered far less severely than did the other vessels of the Navy.

With reference to this disease being of an infectious nature, no infallible signs of such being the case were observed, men as a rule being attacked indiscriminately on board ship without any regard to proximity. It was also remarked that total abstinence from alcoholic liquors afforded no immunity.

In 1891 there were only 3,527 cases of this disease in the British Navy, the total force representing 55,670 officers and men: there were six deaths. The disease appears to have been milder in its effects during this year, the onset more gradual, and the results less permanent in their nature; it was, however, more infectious, and diffused itself with greater certainty amongst bodies of men living in close proximity the one to the other, than in the preceding year. It may here be remarked that the sea-coast towns of tropical America which suffered so severely from influenza in 1865-6 appear to have escaped entirely, while the northern parts of the Pacific Station, which include

Vancouver's Island and British Columbia, were attacked and suffered severely.

In 1892 there were 2,429 cases amongst a total force of 58,330 officers and men. The cases were numerically more evenly distributed over the different stations than in previous years, the Home Station not suffering in so pronounced a manner, although there was a total mortality of five from this affection. In this year one of the training ships, the *Boscawen*, stationed at Portland, which had been for some months previous free from the disease, was re-infected by one boy who returned ill from London; from him the malady spread rapidly amongst the complement of the seaman class, and also infected the guard-ship *Alexandra*, moored in close proximity.

During the year that has recently ended but 1,917 cases occurred amongst a total force of 60,120 officers and men, but by far the larger portion occurred on the Home Station. There were three deaths from this affection. The disease appears to have been generally of a milder nature than that which prevailed in 1892, and seems to have been present, with irregular intervals, more or less throughout the year.

In quitting this section of my paper I would remark that this last epidemic of influenza appears to have been largely carried throughout the habitable world by the mercantile marine. The Swedish vessel *Saga* carried the infection to the Bermudas; a San Francisco steamer conveyed it to the Sandwich Islands; a French mail steamer brought the disease to Zanzibar, causing a heavy mortality amongst the native population; a German vessel conveyed the disease from Sydney to Samoa, resulting in the deaths of some 300 persons.

While revising this MS. a report has been received of an outbreak of the affection at Rio de Janeiro. H.M.S. *Sirius* has been infected, and during August last ninety-two men were placed upon the sick-list out of a crew of 240 for influenza only, causing a loss of 467 days' service to the Crown.

Some five years since it became my duty to give evidence to the Royal Commission on Vaccination, and, as a preliminary measure, to study the history of small-pox as affecting the naval forces of the country.

As early as 1780 the existence of this disease as epidemic in the fleet was so marked as to alarm the Admiralty, as one-fifth of the men then enlisted annually died of this disease. In 1800, small-pox broke out twenty times in the

Channel Fleet alone, compelling its return to port, and seriously crippling its efficiency. The preventive method of Jenner was early recognised by the medical officers, but it was not until 1808 that the first official instructions as to its adoption were issued. This measure assumed simply the form of a recommendation. Further instructions urging its advantages were issued in 1814 and 1825, the latter ordering medical officers of hospitals to advise all patients to submit themselves to its protective influence while under treatment for other diseases.

As the professional records concerning small-pox are very imperfect prior to the year 1856, I will only remark that outbreaks occurred in 1825, in 1830 in the *Blonde* at Malta, and in 1833 in the *Caledonia* at Lisbon.

In connection with vaccination it is recorded that in 1817 Surgeon George Craigie, R.N., proposed to the then Physician to the Fleet to send lymph to the convict establishments in New South Wales in capillary glass tubes hermetically sealed.

In 1854 small-pox was introduced into the Baltic Fleet, and there is every reason for believing that this squadron would have been rendered unserviceable had it not been for the introduction of protective measures into the Navy at a prior date. The disease was first introduced into the *Neptune* by a man who had been under treatment in Haslar Hospital for a pulmonary complaint: there were cases of this disease in the hospital at the time, but the man had not, so far as he knew, been exposed to infection, either at Haslar or in the transport by which he reached his ship. Notwithstanding isolation, the disease spread rapidly in the *Neptune* and to five other ships of the squadron. Altogether, there were 183 cases, of which twelve proved fatal. The disease was ultimately stamped out by hygienic measures. In the remaining twenty-nine vessels, comprising the smaller ships of the fleet, the disease did not appear. The year following, the *Duke of Wellington* and *Arrogant*, also in the Baltic, experienced an epidemic of variola, and between them totalled 135 cases, with eight deaths. The medical officer of the first vessel remarks upon "the extreme difficulty in preventing the extension of zymotic diseases when they make their appearance in crowded ships, for notwithstanding the most careful segregation of the sick within screened berths, it is almost impossible to prevent infection from being carried by the innumerable currents of air set in motion by her movements and the changes in the wind."

Small-pox made its appearance in the south-coast ports of England during the year 1858, and Portsmouth, the naval headquarters, suffered somewhat severely. Men who had been on leave in that town introduced the disease into their ships, one man conveying it to Pembroke, in Wales, and a second to the squadron at Portland. Sporadic cases also occurred in other ships lying at Spithead. The disease remained in evidence during the following year at all the naval ports, but the cases, though numerous, were of a mild nature.

In 1860, however, there was a distinct recrudescence of the disease which assumed epidemic proportions in the ships stationed at Devonport, the crews having been infected from the town, and in the case of the *Royal Albert* by means of workmen while under repairs in Keyham Dockyard.

In these three years, in England and Ireland only, the Navy lost twenty-one men out of 192 cases of small-pox. The Mediterranean Fleet suffered somewhat severely from variola during the year 1860. The disease appeared almost simultaneously in March in the *Agamemnon* at Gibraltar, the *Amphion* at Palermo, the *Hannibal* at Naples, and in five other large vessels. The total number of cases was 153, with eight deaths. In this same year H.M.S. *Leopard* was inoculated at Monte Video by an unvaccinated native servant, and for a time was almost useless as a man-of-war. Out of her small complement thirty-six persons were affected, and the disease did not subside until the ship had landed the sick on a small island near Maldonado, where a temporary hospital, under canvas, was formed.

Malta and the Syrian ports were infected in 1861. At Beyrout H.M.S. *Doris* experienced an epidemic introduced by an unvaccinated person, and returned fifty-eight cases out of a total of 105 on the whole Station.

Small-pox is rarely absent from the large towns on the sea-board of China, and it very frequently exists epidemically amongst the inhabitants. At this period in the medical history of the Service it does not seem a matter of surprise to find that 144 cases of this disease occurred among the ships on that Station. The country, which had barely recovered from warlike operations, was yet in a state of insecurity as far as the European settlers were concerned, and it became a matter of necessity for English men-of-war to remain in localities where the disease was endemic. It was under these circumstances that the *Impérieuse* became infected while in the Peiho River. The disease made its

first appearance in December of the previous year, there having been nineteen cases, with two deaths, up to the 31st December. The last case occurred on 25th January, up to which date there had been 148 cases of the disease in the ship, including those vaguely classified as "variolous fever". The disease existed in every form, from the mildest modified to the confluent and malignant types. An epileptic patient had complete immunity from fits throughout the disease, but when he became quite convalescent they reappeared. In the year 1864 small-pox may be said to have reached its maximum during recent years in the Royal Navy, there having occurred no less than 462 cases, with thirty-three deaths. Of the former, 199 admissions were on the Home Station alone, causing a loss of 5,070 days' service through sickness. Portsmouth, again, was the infecting locality, and during this recrudescence of a then endemic malady, it appeared to be more virulent in its nature than during previous years.

The disease extended widely into the surrounding suburbs, and entered the general wards of the great naval hospital at Haslar. Amongst the vessels which suffered was H.M.S. *Duncan*, which left Spithead for the West Indies early in the year, and with a clean bill of health; eleven days after departure small-pox appeared on board, and lasted for six months. On inspection, eighty-four unvaccinated persons were found on board. The ships in China again suffered severely during this year, returning 150 cases, with sixteen deaths, and a loss by this disease of 3,659 days' service. Nearly one-half of the cases occurred in the *Euryalus*, into which the disease was introduced by an officer who had been infected at Yokohama, in Japan. It was in consequence of the large number of vaccinated persons who had been infected with this disease during the year under review, viz., 274 out of 462 cases of small-pox, that the probability of its protective influence being weakened in course of time was brought to the notice of the Medical Department of the Admiralty, which suggested to their Lordships the importance of compulsory re-vaccination in the public services; and the question was also raised as to the character of the vaccination cicatrix that might be looked upon as affording satisfactory proof that the operation had been successfully performed as far as its protective influence was concerned.

It was also in consequence of the large number of cases of small-pox that occurred on the Home Station that the

first Admiralty Order relative to vaccination was in this year issued to the following effect: "No person is to be entered for the Royal Navy who has not been, or is not willing to be, vaccinated." The result of this order appears to have had a marked effect in diminishing the number of cases of the disease in ships at the Home ports, they falling from 199 to 18, 29, 30, 16, etc., in the following years. From such data we may conjecture what might be the results of the repeal of the Compulsory Vaccination Act in England.

In 1865 the China Station was again brought prominently into notice by an epidemic of the disease which occurred on board H.M.S. *Conqueror*, in which ship there were fifty-nine cases with eight deaths. As is usual in the far East, it commenced in the winter of the previous year, and at Yokohama. At this date small-pox prevailed universally throughout Japan, every adult Japanese being deeply scarred, while mothers and nurses carried about infants in all stages of the disease absolutely indifferent to their sufferings and their power of communicating contagion. Children played about the streets careless of the pustules with which they were spotted, and workmen pursued laborious occupations whilst covered with a mask of scabs. Such was the description of a population then unprotected by vaccination. In a few years these conditions became changed. In the Report of the Health of the Japanese Navy for the years 1892-3, the latest to hand, may be noted that in a force of 9,747 persons there occurred but three cases of small-pox, contracted on shore and not fatal, a marked contrast to the condition of affairs twenty years previous, when 25 per cent. of those attacked fell victims.

A serious epidemic of small-pox was introduced into H.M.S. *Octavia*, then in the East Indies, by a Krooman. He had been sent to the Hospital at Bombay with measles, and returned to the ship, cured, twenty-one days after. Small-pox was prevalent in this town, and it was ascertained that this man had been placed in a ward next to one in which there were two cases of this disease. Four days after he returned he presented himself with confluent variola. Complete isolation was impossible. The epidemic spread at the rate of thirty cases per diem, until 103 were under treatment at one time. In consideration of the alarming state of affairs, the Commodore proceeded to anchor off Butcher's Island, where were several commodious buildings capable of serving as a temporary hospital; here were landed for treatment from day to day 169 cases of

variola. The complement of this ship was 610 souls; of these 589 were vaccinated and 21 not vaccinated. Of those protected, 437 escaped altogether, while 147 were attacked with the disease in a modified form, and five in the confluent form—all recovered. Of the unvaccinated all were attacked with confluent variola, and six died.

While this epidemic was in progress, the 200 men comprising the ship's company of the *Princess Charlotte*, at Hong Kong, were re-vaccinated, four from a healthy Chinese infant, the rest from these four men. The latter existed but in a fool's paradise. The ship visited Hiogo, in Japan; the disease there, endemic, was imported on board, and the day after leaving that port, twenty cases of variola were placed on the sick-list. The disease, as usual, rapidly spread in the ship; she made for Hong Kong, and there landed 116 cases, of whom eleven died. Contrast this vessel with the remainder of the Navy in this year 1867, and her name stands out prominently as exemplifying the uselessness of re-vaccinating from the already immune individual. On H.M.S. *Zealous*, with 565 persons on board, all vaccinated and re-vaccinated, remaining at Vancouver's Island in the midst of a severe epidemic amongst the white and native population, of whom only two recovered out of upwards of 300 attacked, and not contracting a single case of small-pox.

It was in consequence of a recrudescence of this disease in 1871 that the Admiralty issued stringent regulations as to vaccination, the principal bearings of which were to the effect that all persons, excepting officers, were to be re-vaccinated on first entry into the Service, and that a notation to that effect, with the result, was to be inserted on the official papers which accompany each person during his Service career. In this year the largest number of cases of small-pox occurred on the Home Station, principally at Portsmouth and Devonport; the apathy of the civil authorities in the latter town was such that the disease was allowed to spread for months before it was by them officially recognized as epidemic. Only the refusal of the Admiralty to enter local boys in the training ships roused the Corporation to a sense of their duties. Fortunately, the Circular as to re-vaccination had been issued before variola became epidemic in the west country, and thus considerably lessened the number of cases and the mortality. In this year also, small-pox was introduced into Malta by three seamen of a Marseilles ship. The inhabitants, who were not then accustomed to be re-

vaccinated, suffered severely, there being between 5,000 and 6,000 cases during the epidemic, of whom at least one-sixth died. The seamen of the fleet were allowed free intercourse with the shore, and out of a mean force of 4,000 men and officers on the Station there occurred but thirty cases of the disease. After this epidemic subsided re-vaccination was extensively carried out by civil practitioners throughout the Maltese islands, the result to the squadron being that small-pox has practically disappeared from the Mediterranean Station since 1872.

The final epidemic which will be considered is one which occurred in H.M.S. *Boadicea,* in 1879. Towards the close of the preceding year this ship had arrived at Sierra Leone from England, and, as is usual, shipped her complement of Kroomen, native West Africans, for service as stokers and domestics. Small-pox was known to exist in the locality, but, unfortunately, no systematic re-vaccination of the ship's company had been carried out on commissioning. The first case appeared at sea on January 2nd, 1879, in a Krooman, who died ten days after of confluent variola. Every precaution, as far as circumstances would allow, was observed in isolating this case and disinfecting the ship, but on the 20th January fresh cases occurred both amongst white men and Kroomen. On arrival at the Cape of Good Hope the civil authorities refused to allow the cases to be landed; fortunately there was a receiving ship in Simon's Bay, which was cleared and turned into a temporary hospital for the reception of the sick. The last case occurred on 14th February. Of the whole ship's complement of 305 officers and men, thirty-nine suffered from small-pox and twelve died. Thirty of the cases were in white men, and nine in Kroomen. Of the former, twenty-five had the disease in a modified form and one died: of the Kroomen, there were thirty-seven shipped, of whom sixteen had had small-pox. These men suffered severely, no less than seven out of the nine attacked with this disease dying.

Since the year 1879 the stringent regulations in force have been of marked benefit to the Naval Service in checking the spread of this loathsome disease, inasmuch as small-pox has not since that year been able to obtain an epidemic foothold in any one of H.M.'s Ships, in proof of which permit me to refer you to the statistics of this disease which have been submitted. In these you will notice the marked diminution in the mortality columns. Were it not that persons in the Service occasionally escape

perfect re-vaccination the figures in the case column would, I believe, be almost *nil.* It is ten years since Dr. A. Russel Wallace, in his pamphlet on *Registration Statistics and Vaccination,* remarked, "if vaccination is of any use, small-pox should be almost unknown amongst soldiers and sailors, and no soldier or sailor should ever die of it." As far as the Naval Service is concerned the latter part of this remark is fast becoming an accomplished fact—and when the revised Admiralty Regulations upon the subject have had time to reach the distant parts of the world, and the medical officers of ships are able to obtain supplies of fresh calf-lymph on foreign stations, it may fairly be assumed that small-pox in the Navy will be as rare as now is scurvy.

Before quitting the subject of the eruptive diseases it may be remarked that there exists in the Notification of Diseases Act a special clause exempting from its operation any "house, building, vessel, tent, van, shed, or similar structure belonging to H.M. The Queen, or any inmate thereof." Doubtless there may exist very good reasons for the exemption; but this dispensation appears to me to be a retrograde step in sanitation and fraught with a certain amount of danger to the public, on whose behalf the legislature has provided for the prevention of such diseases, as well as to the Naval forces.

There are within the limits of extensive Crown properties, such as the Royal Dockyards, numerous houses, detached and in terraces, whose inmates are in daily communication with the surrounding locality. As matters at present stand there is nothing to prevent any one of these premises from becoming the focus of an outbreak of zymotic disease in a town. For example, a person contracts scarlet fever in London and developes the disease, while in his official residence, at Plymouth. There is no law compelling the head of the family and the medical attendant to notify the occurrence to the local sanitary authority. The disease spreads in the house, attacking the remaining inmates in succession, the premises becoming for the time being an infectious disease hospital in the midst of a crowded locality; crowded at least in the day time during working hours. Communication takes place despite precautions between some of the dockyardsmen, or police, or other employés, and the house. As these persons reside beyond the limits of the establishment there would be a strong probability that they might communicate the disease to their own families and thus establish in the

surrounding districts cultivation centres, if I may so express myself, of infection. Frequent communication also takes place between the dockyards, gun-wharves, victualling stores on shore, and H.M.'s vessels under repair or in commission in adjacent waters, such as are the large training ships lying in the Hamoaze, off Devonport and Keyham yards; in view of which it must be admitted that there is a possibility of ships becoming infected in this manner. The eruptive diseases, especially measles and scarlet fever, will continue to appear in the Returns from the boys' training ships until greater care is taken that they are not communicated from the shore.

At one time these ships at Plymouth, with some 3,000 boys on board, enjoyed perfect immunity from scarlet fever by the simple plan of placing the towns out of bounds, and landing the boys for recreation in some private fields adjacent to the ships and rented for the purpose, on the Cornish side of the river.

In connection with the prevalence of this malady it may be mentioned that on more than one occasion it has been suspected that clothing supplied by local contractors may have contained the germs of zymotic disease, particularly measles; but no proof has as yet been adduced of this statement. It was only last year that the results of infecting a man-of-war at a critical period became prominent. Scarlet fever was introduced, most probably by workmen, into H.M.S. *Hotspur*, then under repairs preparatory to taking part in the naval manœuvres; and to such an extent did the crew suffer that the ship had to be struck out of the list of effective vessels and another take her place. Under private departmental arrangements, medical officers attached to the Naval establishments on shore do intimate the medical officers of health of any case of notifiable disease which may arise during their practice, but only when such cases happen in a locality under a sanitary authority. I submit that it would be for the good of the Naval Service if its medical officers were authorised to call in the aid and professional opinion of the local medical officers of health, especially in cases where it was believed that zymotic disease had been introduced into a previously healthy ship from an infected house on shore. A perusal of the able report upon epidemics of acute pneumonia furnished by Dr. Sidney Coupland to the Collective Investigation Committee of the British Medical Association leaves little room for doubt upon this point. Sokoloff states, from an observation of 2,360 cases, that croupous pneu-

monia is infectious, and can be transmitted in hospitals from patient to patient in a similar manner to erysipelas. Enteric patients have a special tendency to take the infection, and the mortality amongst those who do so is very large. Hirsch states that epidemics of pneumonia have occurred for 300 years, six of which took place in the British Isles, two being at Plymouth, a locality which still furnishes a large number of cases of this disease to the Navy Health Reports. Bauti, in 1877-78 described an epidemic which occurred in Tuscany, and which could not be attributed to atmospheric influences nor to insanitary conditions. In the same year Dr. Couldrey reports in the *Lancet* an outbreak of the disease in the village of Scunthorpe, due in great measure to over-crowding of the inhabitants in badly-built houses. M. Netter of Paris, in 1889, reported a case of a woman who gave birth to a child when she was suffering from acute pneumonia. The child lived a few days, and at the post-mortem examination pneumonia of the right lung was found, as also the characteristic micro-organism of the disease. Again, in the *Lancet* in 1890, Dr. Oliver remarks that in Newcastle a number of cases of pneumonia appeared in the summer months, apparently of an infective and epidemic character. In the same Journal is a report of an epidemic of this malady which prevailed at Scotter, in Lincolnshire. The fatality was great, eighteen out of twenty-nine dying. A number of those attacked had been in close relations with the sick, washing their linen or laying out their corpses, and it is stated that one of the medical men in attendance took the disease and fell a victim. Dr. Bruce Low also reported upon an epidemic of pneumonia which occurred at Raunds, in Northamptonshire, no less than 155 children having been attacked by pneumonia during the period, December 1890 to March 1891.

It is submitted that when pneumonia becomes prevalent amongst a number of persons crowded together in a limited space, and breathing and re-breathing an imperfectly purified atmosphere, the disease assumes a septic form and is communicable, probably by the products of respiration. This form of pneumonia appears to differ, both in its clinical and pathological aspects, from acute catarrhal pneumonia, inasmuch as it is not caused by meteorological conditions, runs a rapid course, and frequently terminates in death. It does not appear to travel from house to house, but that under certain conditions of defective sanitation it may become epidemic in any dwelling, school, or factory, but only

when such defects have lowered the resisting powers of the inmates. If recovery takes place, convalescence is very slow, and is retarded by cardiac weakness, irritability, and a small, rapid pulse.

As far as the Service is concerned, the earliest recognition of this affection appears in the year 1841, when a Naval Medical Officer remarks: "It is not to be denied that there appears to fall upon ships of war occasionally a kind of epidemic tendency to inflammation of the pulmonary organs; but whether this depends on a cause or condition existing within the ship, or amongst the men, it is difficult to form an opinion."

The first example of this disease which has been found in the medical records of the Naval Service, occurred in 1857 in H.M.S. *Conqueror*, then serving on the Mediterranean Station. She carried a ship's company of 900 officers and men, and there is little doubt but that she was overcrowded, as her daily sick-list amounted to thirty-seven. In the *Centurion*, with 740 men there were only two cases of pneumonia during the year, while there were thirty-three in the *Conqueror*. The former ship had but recently been commissioned, the latter had spent two years on the Station, and had by that time a well-trained, well-fed, and seasoned crew. Ventilation and cleanliness were strictly attended to in each ship, but it was remarked by the Medical Officer of the *Conqueror*, "that all wounds and slight injuries received by the men assumed an erysipelatous or inflammatory action."

On the same station there occurred in 1860 a second outbreak of this disease, but not confined in this instance to a single ship. The first to be noticed is the *St. Jean d'Acre*, which was by far the most sickly ship on the station. She arrived in the Mediterranean about midsummer of the preceding year with a ship's company of 815 persons, and soon afterwards they began to suffer from pulmonary complaints. The Ionian islands were visited in the early part of the year, and in March the ship returned to Malta, remaining until June. The weather was fine, the crew had plenty of fresh meat and vegetables, and leave on shore was freely given for recreation and amusement; still the general health, which should have improved, began to decline. The medical officer endeavoured to find out the cause, and directed his attention to the conditions of the decks on which lived and slept the crew. The lower deck, where slept 930 men, was noticed as imperfectly ventilated, especially at night time. The hammock-hooks were placed

ordinarily at only fourteen inches apart—less than the average breadth of the men's shoulders, consequently, while in harbour, when no watch was required on deck at night, and all hands had turned in, they formed a compact mass close beneath the beams, the only air available for respiration being above them. All the ports and scuttles were closed at night, and the hatches covered with gratings. Further, after the men had washed themselves in the morning, this same dirty water was used for cleaning this deck. As the season advanced and the temperature increased, the natural ventilation of the ship was augmented, but there was no interruption to the steady increase of pulmonary disease. . Subsequently extra ventilators were cut through the decks, and the ship sent to cruise at sea, but to no purpose; the disease assumed the proportions of an epidemic, and she was compelled to return to Malta for the purpose of landing her sick at the Naval Hospital. During September and October the weather was fine, and the air cool and less oppressive, but the cases after October 4th increased with great rapidity. On the 9th there were fifteen cases, on the 10th twenty-six, on the 11th twenty-one, and on the 12th seventeen, making in all 102 cases, while the hospital on shore could accommodate no more patients. The ship's company were then removed to the stationary guard-ship, the cases became less numerous, and the epidemic declined. The number of persons attacked was 156 with croupous pneumonia, and a further number of 129 who subsequently developed pneumonic phthisis; of this total number 112 were invalided home. The *St. Jean d'Acre* was taken into dock and thoroughly examined, but no grave structural defect was discovered to account for the epidemic, nevertheless it was deemed advisable that the ship should return to England to be put out of commission. She, therefore, re-shipped her crew, and on Nov. 3rd left Malta. In a month pneumonia again broke out in epidemic form, and on Dec. 29th the ship was compelled to put into Lisbon to land forty-seven cases of pulmonary disease at the Naval Hospital. As to the origin of the epidemic, there can be little doubt that it arose within the ship itself, and was due to overcrowding and defective ventilation, especially in the sleeping quarters of the crew. As originally commissioned, the ship carried 815 men. After arrival at Malta more persons were added, probably native stokers or domestics, until, when the epidemic first made its appearance, there were numbered very nearly 1,000 men and boys on board. As previously re-

marked, 930 of this complement slept on one deck, in an atmosphere loaded with offensive exhalations, and, doubtless, communicated the disease the one to the other through the medium of their respiratory products. As to the respective ages of the men, 511 of the complement were under twenty-six years of age, but no information is available as to the relative proportion of these persons attacked. The *Marlborough*, which carried 1,145 men, and cruised in company with the *Acre*, returned during this period with but seven cases of respiratory disease. She was a well-ventilated ship, and in consequence of carrying distilling apparatus below, the men could not sleep on the lower deck on account of the great heat; they were, therefore, evenly distributed about the ship, with most beneficial results.

About the same time and in the same place the *Cressy* developed the disease, but the cases were by no means so numerous nor so generally destructive of health. She carried a complement of 720, and returns eighty-three cases of infective pneumonia. As the disease subsided after improving the ventilation of the vessel and a re-distribution of the sleeping places of the crew, its origin may be assumed to have been similar to that in the *Acre*. The epidemic did not appear in the remaining thirty-four ships composing the Mediterranean Fleet.

For several years this malady ceased to appear in epidemic form, only sporadic cases occurring in various ships scattered all over the globe; but in 1884 it was remarked that of recent years there had been a tendency towards an increase in the number of cases of diseases of the respiratory system in the ships of the training service, especially in one of the largest and most crowded with boys under the age of seventeen, viz., the *Lion*, at Plymouth. In this vessel there had occurred during the summer months, and in the exceptionally mild climate of that district, twenty-six cases of pneumonia. The year following, there was a still further increase in the number of cases, the *Lion* again being the greatest sufferer, and sending thirty-eight cases of pneumonia to hospital. The medical officer reports the disease to be "asthenic lobular pneumonia in persons suffering from a more or less depraved condition of health in consequence of an unhealthy environment", and further records his opinion that "defective ventilation and overcrowding have been to a great extent factors in the excess of this disease."

In the returns for the year 1886, the largest number of cases of the graver forms of acute chest disease were, as

usual, from the boys' training ships; which circumstance led to a sanitary enquiry and inspection of the vessels, with the result that certain reforms in the system of clothing the boys, cleansing the decks, etc., were carried out. Sundry structural repairs were also effected, but it does not appear that any endeavour was made to increase the ventilation of the vessels, or to give more cubic living space to each individual boy. The following year, however, an improvement took place in the diminished number of cases of respiratory disease in the training ships, but pneumonia still held a prominent position, the death-rate, however, being lessened considerably. Matters improved during the three next years until 1891, when the number of cases increased in the ratio of 62·59 per 1,000 entries, the principal augmentation being in the *Lion*. The reason for this increase was the large number of boys who during this year entered H.M. Navy and overcrowded the training ships. An extra ship, the *Caledonia*, was commissioned and stationed at Queensferry, in the Firth of Forth, and to her the overplus of boys from the Plymouth ships were sent. With a few hundreds she might have remained a healthy vessel, but here again over-crowding shortly took place, with the result that an epidemic of pneumonia occurred of such severity that the physicians of the Edinburgh Royal Infirmary, whither the sick boys were conveyed, became accustomed to describe "Caledonia" pneumonia as the most severe form of that disease. While it is admitted that exceptionally severe weather was present over Great Britain during the winters of 1890-91-92, such meteorological conditions can hardly, in my opinion, be entirely held to have caused this disease to linger in these old, worn-out vessels. Overflow ships were commissioned in 1892 and 1893 to relieve the strain in numbers on the present vessels, and others are now held in readiness for that purpose. As the numbers at present being admitted into H.M. Navy have recently been diminished by the salutary process of raising the physical qualifications to a very high standard, and improved ventilating arrangements have been introduced into all the training ships, particularly Harding's ventilators, which act when the ports are closed at night, there is every reason to think that the past year will find a marked diminution in the number of cases of pneumonia in the nurseries of the Naval Service. In the *Lion*, which in past years has so prominently suffered, each person has a sleeping space of but 168 c. feet; this is insufficient for growing lads if they are to be kept healthy

and strong. It should be possible by perfect sanitation, adequate ventilation, and warm clothing, to largely diminish the loss of service from this disease, so that we could no longer read, as we may in the recently issued Report of the Health of the Navy for the year 1893, that on the Home Station only there was lost to the Crown from these causes work represented by 17,459 days' sickness.

I will terminate this section of my paper with the opinion of Surgeon Whitelegge, who contracted pneumonia on board H.M.S. *St. Vincent* in 1892. He states: "In my own case I attribute the attack to re-breathing the breath of a boy who was suffering from the disease. While examining him I happened to inhale his breath accidentally, and almost immediately felt ill, but recovered in an hour. When the attack developed next day I experienced the same peculiar fœtid taste and smell as on the previous day, and which lasted during the acute stage of my illness."

In concluding this paper it remains but to remark that, as the general knowledge of the fact that the better mode of relieving humanity of the ills to which it is prone is to prevent their occurrence has advanced amongst public communities, so also it may be claimed that the powers having control over the physical well-being of the Navy have not been neglectful of their duties in that respect. The laws of health require to be more strictly obeyed on board ship than on shore; in the latter, abundance of food, water, and fresh air are usually obtainable without much difficulty; in the former condition, human beings are of necessity crowded together in violation of these laws, breathing too often a scanty supply of air vitiated by the retention of their own excretions, subsisting upon an unpalatable diet, their sleep often interrupted, and their minds continually disquieted by their unnatural surroundings. We may, however, fairly claim that sanitary science, especially in the direction of the study of epidemic disease, has had a place in the daily work of the Naval Surgeon for the past fifty years. Curative remedies have largely given place to preventive measures. Upon this point the Admiralty Instructions are explicit. While the Commanding Officer is held primarily responsible for the preservation of the health of the crew, the Medical Officer must, "if he learns on arrival at a place that any disease is prevalent which is likely to prove detrimental to the health of the ship's company in his medical charge, inform the Captain, so that proper measures may be adopted to pre-

vent its occurring or breaking out in the ship or among any of the ship's company." Traditional jealousies and want of confidence on the part of executive officers have gradually subsided and given place to almost the opposite extreme, the influence of enlightened medical opinion in all matters relating to health being eagerly sought and acted upon.

"A TREATISE ON FOOT VAPOUR (DISTEMPER)."

A Translation of a Chinese work on Beriberi presented to the Epidemiological Society,*

By Dr. MITFORD ATKINSON, Superintendent of the Government Civil Hospital, Hong-Kong.

In October 1888, during the course of an enquiry, which as Superintendent of the Government Civil Hospital at Hong Kong I was making concerning an outbreak of Beriberi amongst the water police of that colony, I ascertained that there was in the colony a work on this disease written by a Chinese medical man.

This translation was made for me by Mr. T. Sercombe Smith, Registrar General's Department, Hong Kong, and Dr. Wenyon, Medical Missionary at Fatshan, China.

The author treats of the subject under the following headings:—

1. A brief account of the disease.
2. The colouration of the skin in this disease.
3. Method of taking the pulse.
4. Recurring "Keuk Hi".
5. The prevalence of the disease in summer and spring.
6. The disease due to local miasma.
7. On the resemblance between this disease, ague, and paralysis.
8. Minute precautions.
9. Of the betel-nut cure.
10. Cautions with regard to this disease.
11. Important prescriptions.
12. Directions for prescribing medicines for "Keuk Hi".
13. Directions how to detect by the pulse the coming on of the disease.
14. Indications of methods for treating the incipient stages of the disease.

* Unfortunately this interesting document is too voluminous for insertion in the *Transactions*, but it is felt that Dr. Atkinson's summary will sufficiently indicate to members the nature of the contribution, which, together with the original work in Chinese, may be obtained from the Honorary Secretaries.

15. Indications for treating an advanced stage of the disease.
16. Indications for treating the disease when it is beginning to oppress the chest.
17. Indications for treatment of this disease when it has first attacked the chest.
18. No cure when the disease obtains firm hold of the chest.
19. How to distinguish and prescribe for the eight forms of "Keuk Hi."

By the term "Keuk Hi" is meant Beriberi. It is evident that this disease has been known to the Chinese for many years, first under the name of "Woon Fung" (slowly progressive palsy), and, secondly, under the name "Keuk Hi" (Foot Vapour).

Amongst all this mysticism and bewildering obscurity they have at any rate recognised the principal symptoms of the disease, viz., "numbness, accompanied with swelling and pain, relaxation and incontractility of the muscles"; they distinguish between an acute and a chronic form corresponding to our Beriberia hydrops and Beriberia atrophia; they are also probably correct in ascribing it to an emanation from the sub-soil, or, as they term it, "the moist vapour of spring".

They also recognise the benefit to be derived from a change of climate.

We cannot, I am afraid, expect much more from a people who are ignorant of the first principles of physical science, and destitute of the spirit of experimental inquiry.

With regard to their prescriptions, which are almost impossible to correctly translate, they contain many ingredients, seldom less than nine or ten, including such incongruous things as tiger's bones, "cock-crowing" powder, etc.

As the Chinese convert almost every organised substance into human food, they make similar exactions on the animal and vegetable kingdoms for the cure of human maladies.

USE OF EUCALYPTUS OIL AS A DISINFECTANT IN SCARLET FEVER.

By JOSEPH PRIESTLEY, B.A., M.D., D.P.H.

(*Read: 15th March* 1895.)

Mr. President and Gentlemen,—In the first place allow me to thank you, Mr. President, and the Members of the Epidemiological Society, for the honour of being asked to read a Paper before your Society.

It will be within the recollection of some that on 12th March 1890, Mr. J. Brendon Curgenven of Teddington read a paper before this Society on the use of eucalyptus oil in scarlet fever and other infectious diseases—a paper in which, if the author will allow me to say so, the praises of the eucalyptus were somewhat extravagantly sung, and in which conclusions were somewhat too hastily drawn—in my opinion at least—from very few cases. The same praises were sung in the same extravagant manner before other learned societies, viz., (*a*) the International Congress of Hygiene and Demography (on the 14th August 1891); and (*b*) the Medical Officers of Schools Association (on 13th January 1893); in the public press, medical and lay; and in pamphlets; whilst the chorus was taken up by the manufacturers of the special drug named, and shouted throughout the length and breadth of the land. Practically, however, the subject did not "catch on", and, with a few exceptions, which I shall mention, little or nothing further has been heard of it.

Under the terms "extravagant praises" and "too hastily drawn conclusions" may be included, amongst others, the following extracts from Mr. Curgenven's papers:—

"Scarlet fever and other infectious patients are to be treated in their own homes without risk, or the extension of the infection, to those around them, with the result that our large fever hospitals (in London and elsewhere) will stand empty, and consequently the present heavy burdens upon the ratepayers in connection with the isolation and treatment of infectious diseases will be relieved, and

enormous amounts of money saved; for the vapour of eucalyptus destroys not only the spores of the pathogenic bacilli of scarlet fever and other infectious diseases, but the bacilli themselves, after they have set up the pathological trains of symptoms constituting the fevers, and are being rapidly generated in the blood and tissues of the patients. That is to say, the skin, mucous membranes, secretions and breath of the patients—not to mention the bedding, and even the rooms themselves—are disinfected, so that the patients cannot communicate their diseases to others, although daily in the same room or rooms. In this way, in the case of scarlet fever, the disease is arrested if eucalyptus inunction is used during the initial stages of headache and vomiting, or during the first day of the fever, with the result that no rash appears and no desquamation follows, the patient being restored to health in twenty-four to seventy-two hours; whilst even if this treatment is not commenced until after the first day or two, the sequelæ (suppurative or otherwise) are lightened, if not wanting altogether, the symptoms are mitigated, duration of the fever shortened, and the mortality considerably lessened, if not entirely prevented." At least so says Mr. Curgenven in his various published papers.

Since I have been connected with the public health service, I have always felt that there was room for improvement in the treatment of scarlet fever patients; it seemed to me that the long stage of desquamation, during the greater part of which the hospital patients are to all intents and purposes convalescent, was (to say the least) hard lines upon the general body of ratepayers, and that therefore all scientific endeavours should be directed towards the shortening of this desquamation period, or towards the rendering of the desquamation itself in some way or other non-infectious. The average length of stay in hospital for scarlet fever cases varies in different institutions, *e.g.*, in Leicester Borough Hospital it is about six weeks, as compared with seven to eight weeks or longer in the hospitals of the Asylums Board.

Then again, despite such a lengthened stay in hospital, patients when sent home appear (in a proportion of cases at least) to give rise to the disease in others. These are the so-called "return" cases, *i.e.*, cases that are apparently, in some way or other, connected with the return of convalescents from infectious diseases hospitals to their homes; and their occurrence is disheartening when all possible precautions are taken in the way of keeping the

patients isolated in hospital until every visible trace of peeling is gone, and in the way of scientifically disinfecting clothes, etc.

As the result of my own investigations into "return" cases of scarlet fever, I have long felt that perhaps the desquamating stage is not their *vera causa*, the peeling skin not being so infectious as is generally supposed.

It is true that we, as health officers, are satisfied if we discover an antecedent case of peeling in the neighbourhood of a localised outbreak of scarlet fever; but it is equally true that in Leicester, when at the time of the outbreak of small-pox not less than 120 children in various stages of desquamation after scarlet fever were sent to their homes, no single second case occurred at any of these houses, although I carefully watched them for three months. Surely this was not a pure coincidence?

My own line of reasoning is as follows: Scarlet fever is an infectious disease due to the entrance into the system of a micro-organism—the micrococcus scarlatinæ of Klein, the bacillus scarlatinæ of Edington, or other germ—the point of attack being in all probability the throat. On entering the body at this point, and incubating there for a certain length of time, the poison (be it the germ or its products) gives rise to certain changes in the blood, with a resultant train of symptoms, *e.g.*, temperature and rash followed by peeling of the epidermis. The simplest explanation is that the rash, *i.e.*, hyperæmia or congestion of the skin, causes its outermost layers to die, to be hereafter cast off as waste products. No definite, generally accepted, specific germs have as yet been found in this shed skin, nor have any cultivations been made therefrom as far as I know; indeed such skin is dead, and it has therefore seemed to me a little difficult to understand how such dead skin could so readily give (as it is generally stated) rise to the disease in others. I do not of course definitely state that it does not, but it has not yet been satisfactorily proved that it does; whereas experience, I think, is tending to prove that scarlet fever is extremely infectious in its early stages, *i.e.*, from the moment that sore throat and vomiting appear. Small-pox is undoubtedly infectious in its very early stages, as are also measles, diphtheria, influenza, whooping-cough, and typhoid.

The fever stage in scarlet fever lasts one to two weeks; the stage of desquamation six to seven. Presumably, when the fever stage ends, the germs and their products have ceased to act deleteriously, and *theoretically* therefore all

that is required is to render aseptic during the fever stage the blood, secretions, mucous membranes and skin, breath, etc. This Mr. Curgenven claims *practically* to have done —a claim not yet seriously contested, and when my attention was first called to his eucalyptus treatment, I may perhaps be described as almost prejudiced in its favour. At any rate my own views in regard to scarlet fever infection were such as I have just described, so that no one could say that I was prejudiced against such a line of treatment or disinfection. I read all Mr. Curgenven's papers, and even after allowing a deduction for the praises naturally lavishly bestowed by a father upon his own offspring, I felt that I had at last found my long-looked-for and long-wished-for remedy, by means of which I should be able to save my authority a considerable sum of money, whilst at the same time a much larger number of patients suffering from scarlet fever could be treated in my present hospital accommodation per year.

Considering that Mr. Curgenven unfortunately had to base his conclusions in regard to scarlet fever upon twenty-six cases, I anxiously waited for further statistics from other medical men, but none came. I decided therefore, and through the courtesy of my committee was enabled, to try the treatment at the Leicester Borough Hospital, seeing and treating all the cases myself. In fact, I was able as medical superintendent to treat the cases, and then as medical officer of health to watch that no evil results arose therefrom in regard to the spread of the disease in the town.

I propose now to lay before you all my facts, so that you may draw your own deductions therefrom, contenting myself with reminding you that the comparatively few cases (120) that have been eucalyptus-treated by me do not warrant any very definite conclusions being drawn, but may perhaps tone down somewhat the exaggerated statements of others (on both sides), and be an incentive to those of you who have opportunities to do so to officially give this treatment a trial, and to publish careful notes. I personally feel, as no doubt others do, that the subject is one that ought to be stringently and scientifically criticised, and a definite opinion "for" or "against" come to. Mr. Curgenven's paper, having the honour of being printed in your *Transactions*, and therefore practically "passed" by the Epidemiological Society, holds the field, and his conclusions, until rebutted, must be accepted.

I have carefully followed out the treatment as suggested,

personally supervising it, and have only included amongst my cases those patients who showed the rash well developed, so that not only was the diagnosis certain, but the disease had not advanced beyond its early stages, *i.e.*, first or second day. It would be manifestly unfair to include cases with only initial symptoms, *e.g.*, sickness, headache, and sore throat, as whatever the result of the treatment might be in such patients, it could never be definitely stated that they were genuine cases of scarlet fever. For I confess that I am one of those who deny the existence of scarlet fever without rash and subsequent peeling (branny or otherwise), though I recognise the so-called pseudo-scarlatinal cases, which are sometimes met with in scarlet fever infected houses, or in times of epidemics, but which are not, in my opinion, genuine cases of scarlet fever.

My mode of treatment has been as follows:—Careful rubbings with the oil over the whole body three times a day for three days, followed by one rubbing (after a warm bath) daily for seven days. The disinfectant is sprayed also into the patient's throat, mouth, and nose, and diffused into the air of the room. No eucalyptus has been administered internally beyond what has passed down into the stomach during the spraying process. Further, sequelæ and complications as they arose were treated with the usual remedies, with which you are all well acquainted—in addition, of course, to the eucalyptus oil. The oil that I use is Tucker's oleusaban "A" disinfectant, which is stated to be the essential oil of eucalyptus globulus with thymol and other camphors and aromatic antiseptics in solution in definite proportions—freed from most, if not all, resins and aldehydes by distillation and redistillation. There is stated to be a fluid hydrocarbon (eucalyptene) holding in solution an oxidised hydrocarbon isomeric with camphor, viz., eucalyptol. The eucalyptus oil is volatile and diffusible—evaporating readily—and so offers little or no obstruction to the pores of the skin; and it does not turn rancid. It is stated further to contain no fixed oil, no water, and no alcohol, and to be a non-poisonous, non-staining, and non-irritating germicide, antiseptic, and antiferment, according to the researches of Omelchenko, Bucholtz, Siegen, Mayo-Robson, Lister, Schultz, Klein, Pitzer, Hirst, and others.

I have compared the 120 patients treated with eucalyptus with 161 treated in the usual orthodox way, and at the same time, in the same hospital, and under precisely the same conditions, *i.e.*, I have only included cases that showed

the rash well out and were in the early stages of the disease. Further, all my patients were taken from the same epidemic (a declining one), and in rotation as they were notified, so that no selection of cases took place. In short, as far as possible, I chose my two sets of cases so that they practically agreed as to age, constitution, conditions of life, stage of disease, and severity of attack—the only difference, therefore, being one of treatment, *i.e.*, eucalyptus *v.* orthodox or ordinary. The majority of the cases were mild. The total 281 cases were the only suitable ones admitted to hospital during 1894.

My results may be tabulated as follows:—

	Eucalyptus.		Ordinary.
No. of cases of scarlet fever treated, and number of deaths.	120 cases and 2 deaths = 1.6 per cent.	...	161 cases and 7 deaths = 4.3 per cent.
Length of stay (in days) in hospital:—			
Non-fatal cases	34.4	...	42.7
Fatal cases	8.5	...	13.4
Length of interval (in days) between admission, *i.e.*, commencement of the treatment, and normal temperature, *i.e.*, the cession of all fever	11.4	...	10.3
Percentage of complications and sequelæ	20	...	27.3
Serious	79.3	...	83
Trivial	20.7	...	17
Percentage of "return" cases, *i.e.*, of all those treated with eucalyptus and discharged from hospital during 1894	3.9	...	5.0
Calculated on special cases treated	= 4.2 per cent.	...	= 6.4 per cent.
Particulars as to complications and sequelæ:—			
Serious—			
Rhinorrhœa	2	...	1
Otitis	9	...	7
Phagedæna pharyngis	2	...	4
Acute rheumatism	2	...	4
Abscesses	4	...	1
Albuminuria	2	...	14
Uræmic convulsions	0	...	3
Pneumonia	1	...	4
Acute tuberculosis	1	...	2
Hæmaturia	0	...	1
Meningitis	0	...	1
Anasarca	0	...	2
Trivial—			
Tonsillitis	2	...	2
Pityriasis	1	...	0
Urticaria	1	...	2
Epistaxis	0	...	1
Erysipelas	0	...	1
Re-infection, *i.e.*, secondary rash	2	...	3

It must be stated that with twenty-three exceptions the

eucalyptus cases were kept in hospital until quite smooth, *i.e.*, until all visible peeling was gone. If we take the ninety-seven cases that were thus kept in hospital until all peeling was finished, we find the average stay in hospital of each patient was 35.6 days, whilst the average stay for the twenty-three patients who were sent out still peeling on the feet was 32.4. It makes, therefore, little difference, so that we may treat of all the eucalyptus cases together, with the understanding that *practically* each case was detained in hospital until smooth. I do not feel justified in sending the patients out into the town at the end of ten days, as suggested by Mr. Curgenven; as medical officer of health I feel chary of doing so, of being party to such an arrangement; though as medical superintendent I sent twenty-three patients out with peeling feet (the rest of the body being smooth) without any bad results accruing—without, in short, coming into collision with my other self, the medical officer of health!

The five "return" cases in connection with eucalyptus patients have reference to those who were perfectly smooth —all peeling having ceased—though I may mention that two were connected with patients who were discharged (at the request of the parents) with running ears, and we must all admit that a suppurating middle ear is *theoretically* an ideal cultivation medium for germs, and *practically* has been shown to be so by the interesting bacteriological investigations of Dr. Frank Blaxall, who shows that the streptococcus pyogenes, the staphylococci albus and aureus, etc., are constantly found there, whilst Fraenkel's diplococcus pneumoniæ and Friedlander's bacillus pneumoniæ have also been found by other observers. Of the five "return" cases, the patients in connection with whom they had apparently arisen had been thirty-five, twenty-three, twenty-five, forty-four and thirty-four days respectively in hospital, whilst the intervals elapsing between the dates on which they returned home and the "return" cases sickened were five, fifteen, six, eight and twelve days respectively.

Taking my own eucalyptus statistics, then, it would appear that there were, in those particular cases, in favour of the eucalyptus: (1) a lower death-rate; (2) a shorter stay in hospital; (3) slightly fewer complications of a serious nature; and (4) fewer "return" cases; whilst the process of desquamation was certainly hastened, with, I may add, a slightly increased epithelial loss. The stage of fever was practically the same in both sets of cases—if anything slightly lengthened in the eucalyptus ones, and I

would mention here that the fever was not supposed to be gone until normal temperature had been not only attained, but also maintained. Two doubtful cases of scarlet fever out of five that were put into the eucalyptus ward and treated, developed a rash, sore throat, and temperature in four and nine days respectively. Other results noticed were: stimulation of the skin with feeling of warmth to patient, heightening of the colour of the rash, slight cough, slight suffusion of conjunctivæ and tendency to sleep after the application, pulse softer and less rapid; whilst the smell of the oil was but slightly objected to.

As to the cost of the treatment. My figures work out to (about) 2*s.* per patient, and taking the average number of rubbings as sixteen, it is equal to 1½*d.* to 2*d.* per application (about an ounce being used each time). Stay in hospital, however, is shorter, and so the hospital maintenance expenses per patient are less. In Leicester during the last nine years the average weekly cost per patient is 16*s.* 2½*d.*, and this I take in connection with an average stay in hospital of 42.7 for orthodox, as against 34.4 for eucalyptus, treated cases. There is thus a saving of a week's expenses, *i.e.*, 16*s.* 2½*d.*, whilst the oil costs 2*s.*—leaving a balance of 14*s.* 2½*d.* in favour of each patient treated with eucalyptus, even when such patient is kept in hospital until practically smooth.

So much for my own results, and now a few words about those of others.

1. Mr. C. Knox Bond, then resident superintendent to the City of Liverpool Fever Hospital, in the *Lancet* of 6th June 1891, gives some statistics in regard to the cases treated there during 1890. There were in all 305 cases of scarlet fever, and of these, forty-seven (forty-four being consecutive cases) were treated with eucalyptus oil, which was administered internally in two to five minim doses every four hours in water and mucilage, for two weeks and upwards—the oil being also at times used as a lotion with water, or in spray, in cases with severe throats, or as a douche in cases with nasal catarrh. No inunction, no smearing over the skin, of the patients was tried, nor is any mention made of the kind of eucalyptus oil used. It will, therefore, be seen and admitted that these statistics are not comparable with my own, nor with those of Mr. Curgenven. I will, however, give them for what they are worth, more especially for the logic in the conclusions drawn!

"As a curative agent", Mr. Bond says, "the internal

administration of eucalyptus possesses no value, neither mitigating the severity nor modifying the course of the disease; whilst it fails to prevent serious complications and sequelæ. Therefore", Dr. Bond continues (and here comes in the logic), "I am sceptical as to the use of eucalyptus rubbings"!

Of the forty-seven cases, four died = 8.5 per cent.; in twenty cases, *i.e.*, 42.5 per cent., there were complications and sequelæ as follows:—Acute nephritis, 5; cervical cellulitis, several; albuminuria, 16 (*i.e.*, 34 per cent.); diphtheritic ulceration of throat, 2; multiple pyæmic abscesses, 1; whilst rheumatism, pericarditis, and endocarditis existed, but it is not stated in how many cases. Vomiting occurred in several children, whilst the efficacy of his eucalyptus as a deodorant must have been undoubted, for it is even admitted by Mr. Bond.

2. A somewhat severe criticism of the treatment appeared in a paper read before the Medical Officers of Schools Association on 27th June 1894, by Mr. H. G. Armstrong, medical officer to Wellington College.

This paper appears to have been written for, and at the suggestion of, the council of the society, who by resolution ordered it to be printed, no doubt with the intention of ridiculing the whole subject of antiseptic (eucalyptus) inunction. The title of the paper was "The Treatment of the Exanthemata by *so-called* Antiseptic Inunction", the conclusions being practically drawn from a series of answers obtained from members of the Officers of Schools Association and medical superintendents of different metropolitan fever hospitals—the opinions merely (some backed by actual experience) of twenty-three medical men.

I shall only quote from this paper in so far as it relates to scarlet fever, and I commence with Mr. Armstrong's own words: "I have not had any opportunity", he states, "of trying the oleusaban treatment in regard to scarlet fever"!

Mr. Armstrong, I would remind you, is the author of the paper, and his main object apparently is, as I have said before, to ridicule the treatment, and this he attempts to do in his opening remarks by comparing it with that of Herr Dr. Schneemann of Hanover, published in 1848. On reference to Schneemann's paper, I find his treatment as follows:—Whole body (except the face) to be anointed twice daily for three weeks, and once a day during the fourth week, with lard. Patient to be kept in a room well ventilated at a low temperature (not more than sixty-one

degrees F.), and no medicines to be given; patient to avoid going to bed, and to move about as much as possible, even out of doors on the tenth day; the clothing not to be changed. For this it was claimed that there was no regular desquamation. This treatment of Dr. Schneemann is summed up by Mr. Armstrong in the words "the bacon fat method", and I merely mention this to show the spirit in which the eucalyptus is assailed by the author whose paper we are now dealing with.

The answers of the twenty-three medical gentlemen refer to scarlet fever, and may be classified as follows:— (*a*) In regard to antiseptic inunction, is it a preventative of infection? Nine say "yes" or "probably"; three only say definitely "no"; two are doubtful on the point; whilst nine, having had no experience, rightly decline to express any opinion. (*b*) In regard to antiseptic inunction, does it exert any specific power over the disease, in the way of shortening its course and preventing complications? Seventeen say definitely "no"; four say "yes"; two express no opinion; whilst of the seventeen who say definitely "no", one goes so far as to say that it is worse than useless, and another makes the pointed observation that "the only case on which the treatment was tried. died". In respect to this last, we must remember that one swallow does not make a summer.

Mr. Armstrong himself, though he admits (as we have seen) that he has not had any opportunity of trying the treatment in regard to scarlet fever, compares some of his own cases treated in another way with those treated by Mr. Curgenven with eucalyptus. Such a comparison is of course useless and unscientific.

The conclusions finally drawn, and apparently adopted by the association, are expressed by the author as follows:— "I have but ill succeeded if I have not shown that an antiseptic inunction does not exert any specific power over infectious diseases; that it has but little, if any, power in preventing the spread of infection; whilst the cases thus treated are more subject to complications." As regards scarlet fever, at any rate, I am bound to confess that Mr. Armstrong has, to quote his own words, "but ill succeeded" in condemning the eucalyptus treatment. It may be mentioned also that Mr. Armstrong abuses the oil itself by stating that it does not completely evaporate, but on the contrary forms an impermeable covering or varnish, which is dangerous to the health and life of a patient treated with it, whilst the method is extremely troublesome to carry

out, and is dangerous owing to the inflammability of the oil.

In the remainder of his paper, the incubation period and infectiousness of scarlet fever are dealt with in an interesting manner, it may be noted.

3. Mr. R. M. Bruce, of the Western Fever Hospital, is quoted at length by Mr. Armstrong, so that I propose under this third heading to give his statistics in detail. In 1892 sixteen patients were treated by him with eucalyptus oil, being rubbed from head to foot twice daily for fourteen days. The stage of the disease was:—In six cases the first day of the rash; in nine cases the second day of the rash; and in one case the fifth day of the rash. Desquamation ran the usual course in every patient, whilst five only of the cases were uncomplicated = 31.2 per cent. Eleven (*i.e.*, 68.8 per cent.) showed complications, thus:—Otorrhœa, 4; albuminuria, 2; simple adenitis, 1; suppurative adenitis and acute nephritis, 2; otorrhœa, suppurative adenitis, lobular pneumonia and albuminuria, 1; otorrhœa, adenitis and nephritis (with death on the fifty-eighth day), 1.

In connection with these statistics it may be mentioned that Mr. Curgenven too gives the statistics of a medical superintendent of a fever hospital, in which sixteen cases were also eucalyptus treated, but in which the results were more satisfactory, thus: 10, *i.e.*, 62 per cent., had no sequelæ; 6, *i.e.*, 38 per cent., had complications, viz., 3, *i.e.*, 19 per cent., acute nephritis; 2, *i.e.*, 12 per cent., varicella and pertussis; 2, *i.e.*, 12 per cent., adenitis and abscess; 3, *i.e.*, 19 per cent., rhinorrhœa and otorrhœa. In other words, 30 per cent. had suppurative complications, and one died on the forty-seventh day of lung disease. It has been suggested that these two sets are the same cases; let us hope not, for the reputation for accuracy of somebody.

4. Dr. R. D. R. Sweeting (late medical superintendent of the same Western Hospital) published in the *Medical Magazine*, for May 1893, a somewhat caustic criticism of Mr. Curgenven's treatment to the effect that "at present the advocacy of eucalyptus in scarlet fever and other diseases rests on a crude generalisation evolved from ill-considered data of the most incomplete and imperfect nature". This may, or may not, have been true, but it certainly was not warranted by the facts elicited in the paper on the other side. In fact, Dr. Sweeting quotes only the same sixteen cases mentioned above, and leaves us to infer therefore that he had not made himself thoroughly

acquainted with *all* Mr. Curgenven's papers, whilst it is possible that he himself had never treated a single case with eucalyptus. Dr. Sweeting's paper, however, is interesting on account of his statement that "return cases ought to be impossible at a well-administered hospital—not a single well-authenticated case (he states) having occurred in his long experience". The Asylums Board Reports do not, I think, agree with this, whilst Nottingham even boasts of what, in the opinion of her medical officer of health, is a low percentage of "return" cases, viz., 3 to 5.5, and would certainly not like it to be inferred therefrom that her hospital was not well administered.

5. Dr. Edward Little has used the eucalyptus treatment for scarlet fever cases at the Wimbledon Hospital for Infectious Diseases, and in his own private practice, with the following results:—Process of desquamation hastened; many of the cases were free at or under four weeks—in none was desquamation prolonged beyond five weeks; no "return" cases; whilst two or three children not suffering from scarlet fever have been put into scarlet fever wards with patients for ten days, and have not contracted the disease.

6. Dr. Thresh, medical officer of health to Essex County Council and of Chelmsford R. S. A., in the *British Medical Journal* of 4th March 1893, says "the eucalyptus has repeatedly failed to prevent the spread of the infection of scarlet fever". Unfortunately, no particulars are given, and this applies also to the somewhat general statements in favour of the treatment of other medical men, *e.g.*, Mr. Cartwright (Leintwardine), Mr. Garland (Yeovil), Mr. Ellsom (Whitwell), Dr. Hayward (Haydock), Mr. Sunderland (Bexley); whilst Mr. S. Peake of Shepherd's Bush gives a few particulars in the *Medical Magazine* of August 1893, in regard to six cases treated by him antiseptically with eucalyptus oil. Mr. Peake practically endorses all that Mr. Curgenven says (as quoted by Dr. Sweeting), with the single exception of the statement that the treatment *in all cases* prevents suppurative complications.

I do not touch upon the evidence of clergymen, headmasters of schools, matrons, or even the sanitary inspector of Enfield. Such evidence will not further the object I have in view, viz., to thoroughly, but scientifically, investigate this eucalyptus treatment. Such emphasis has, however, been laid upon the Enfield evidence—that of the sanitary inspector who distributed the eucalyptus to fifty-four scarlet fever infected houses—that I may, perhaps, with advantage say a word about it. Eleven secondary

cases arose at intervals, varying from six days to eight weeks, so that Mr. Curgenven thinks that such evidence goes to show the efficacy of the treatment as a preventative of infection. The medical officer of health of Enfield (Dr. Ridge) does not, however, apparently think so, for he is reported, in the *British Medical Journal* of 4th March 1893, to say that "we have used eucalyptus, but without evidence of the value said to attach to it".

Dr. W. A. Jamieson of Edinburgh has much to say in favour of the principle of the antiseptic treatment of scarlet fever, but the antiseptic he uses is not the one we are to discuss to-night, so that I must pass him by. Suffice it to say that he does not approve of either Mr. Curgenven's treatment or his statements.

The same may be said of Dr. Wm. Collier of Oxford, who wrote to the *British Medical Journal* of 18th March 1893, on the same subject of antiseptic inunction.

Before leaving the subject I feel I must say a word or two about the difference between hospital and home treatment of scarlet fever—against the former of which Mr. Curgenven uses some rather strong language, calling attention again and again in the course of his writings to the disastrous results arising from crowding in hospitals. For the Asylums Board hospitals he gives 7.1 per cent. as the scarlet fever mortality rate, as compared with 2.1 per cent. for home-treated cases, and compares these results with statistics from Enfield—147 cases treated at home, in the majority of which eucalyptus was used, with a death-rate of 1.42 per cent., and 85 in hospital with a death-rate of 3.26 per cent.

Statistics require careful handling, but I would mention that in Leicester during ten years (1883-92) 7,612 cases of scarlet fever have been notified, and of these 4,488 were treated in hospital (death-rate 3.6 per cent.), and 3,124 were treated at home (death-rate 8.2 per cent.).

There is no necessity to cry down hospitals and isolation in order to cry up eucalyptus oil.

In conclusion, I trust it will not be thought that I have been too severe upon Mr. Curgenven *and others*, but that it will be understood that my only desire is to get at the truth of this eucalyptus treatment, and to inspire my colleagues to scientifically investigate it, at least in regard to scarlet fever. There is, I think, more in it than the "bacon fat" of Mr. Armstrong, though less than the "hospitals disestablishment" of Mr. Curgenven.

In medio tutissimus ibis.

IMMUNITY.

By J. W. WASHBOURN, M.D., F.R.C.P.

(*Read: 19th April* 1895.)

IMMUNITY is one of the most interesting questions in the whole range of pathology. Not only is the subject of scientific interest, but it is also of great practical importance. The antitoxine treatment of diphtheria is entirely due to the knowledge obtained from the scientific investigation of immunity, and there can be no doubt that this is but the commencement of an entirely new method of treating all infectious diseases.

The problem of immunity is a complex one, and can only be solved by a careful consideration of all the facts concerned in the diseases due to the invasion of the body by bacteria.

Mode of Action.—The first point to consider is the way in which bacteria act. It may be stated that all pathogenic bacteria produce their ill effects by means of the poisons they elaborate; these poisons are called toxines. The proof of this is shown best in the case of the diphtheria and tetanus bacillus. In artificial cultivations these bacilli produce toxines, which, when introduced under the skin of susceptible animals, produce exactly the same symptoms as are caused by the introduction of the living bacilli. Both these toxines are exceedingly poisonous, especially that of tetanus. A hundred-thousandth part of a cubic centimetre of tetanus toxine is sufficient to give tetanus to a mouse. Many pathogenic bacteria, on the contrary, produce very feeble toxines. The pneumococcus may be taken as an example. Large quantities of filtered or dead cultivations may be injected into the circulatory system of susceptible animals, such as rabbits, without producing any ill effect.

Now, the diphtheria bacillus and the tetanus bacillus, when inoculated under the skin, do not invade the circulatory system, nor the internal organs; they only multiply at the spot of inoculation, elaborating toxines which, after absorption, produce the characteristic symptoms of these

diseases. We may look upon diphtheria and tetanus as types of *toxic* diseases.

And here we may remark that tetanus occupies a peculiar position, for it has been found that tetanus spores, when freed from toxines by washing, or in other ways, produce no symptoms when injected under the skin. In order for the spores to germinate and produce their toxines, the tissue must first be injured, or other bacteria, harmless in themselves, must be simultaneously injected. This association of bacteria is an important factor in many diseases, and must not be lost sight of in the consideration of their pathology.

In contradistinction to *toxic* diseases there are others which may be called *septic*. A good example is the disease caused by the pneumococcus in the rabbit. The cocci, which, as we have already learnt, produce but feeble toxines, rapidly invade the whole body, and the blood and organs are found crowded with them after death.

There are all degrees between the typical *toxic* and *septic* diseases we have quoted. The bacillus of septicaemia haemorrhagica produces a powerful toxine, and nevertheless multiplies in the blood of infected animals. The disease produced by one and the same bacterium may sometimes take on a septic and sometimes a toxic type. The pneumococcus generally produces a septic type of disease in the rabbit; but at times it produces a toxic type, the cocci only multiplying at the spot of inoculation without invading the blood. Death is then due to a poisoning with the toxines.

These considerations show that in the study of immunity we have to consider two main points. The one is the power of the body to destroy bacteria, or to inhibit their growth; and the other is the power of the body to resist the effect of the toxines. In the case of septic diseases the former factor, and in the case of toxic diseases the latter factor, is the more important. Immunity may depend upon either factor, or upon a combination of both, and this is one of the reasons why the problem is such a complex one, and why no one theory is applicable to every case.

Immunity may either be natural or acquired.

Natural Immunity.—Many animals are naturally immune to bacteria which are exceedingly pathogenic to other animals. Fowls, frogs, and white rats are immune to anthrax; and birds generally are immune to the pneumococcus. A natural immunity can be overcome by various agencies which lower the vitality of the animal.

Fowls can be infected with anthrax when exposed to the depressing influence of cold; and many other instances might be given. In some cases a natural immunity is chiefly due to a tolerance of the toxines. Fowls are not affected by large doses of the tetanus toxine, just in the same way as they are insusceptible to large doses of morphia. Mice are not affected by large doses of the diphtheria toxine.

Acquired Immunity.—An *artificial* immunity can be acquired in various ways. The method which has been employed for a long time is that of submitting the animal to a mild form of the disease by, in fact, the process of vaccination. This can be effected by inoculation with attenuated cultivations, or by inoculation with very minute quantities of virulent cultivations. The anthrax bacillus can be attenuated either by cultivation at a high temperature, or by the addition of various antiseptic substances to the cultivating media. Inoculation with such attenuated cultivations produces a local reaction attended with pyrexia, from which the majority of the animals recover. After recovery the animals will resist inoculation with a virulent cultivation.

A similar result can be obtained by the injection of non-fatal doses of the toxines; and this indeed is what would be expected from our knowledge of the mode of action of bacteria.

As a rule, immunity conferred by the injection with toxines is not so durable as that produced by inoculation with living cultivations.

There are several points that must be considered in producing immunity in this way. In the first place, it takes some time for immunity to be established, and secondly, the immunity will only last for a certain time. If the animal is inoculated too early after the preventive inoculation, it will succumb; and so will it if the inoculation is deferred too long. The period depends upon the nature of the disease, and also upon the effect produced by the preventive inoculation. The greater the effect the longer will it take for immunity to be established, and the longer will it last. Although there is a close relationship between immunity acquired towards living bacilli and to their toxines, yet these are not always in direct relation.

It is easier to render an animal immune to fatal doses of living bacteria than to fatal doses of the toxines.

It may be stated as a general rule that when an animal has been rendered immune to the toxine of a given bacterium, it is also immune to the living virus. For

example, when by previous treatment an animal will withstand a fatal dose of the diphtheria toxine, it will also withstand inoculation with a fatal dose of the living bacteria. The converse of this rule does not, however, hold. An animal may be rendered immune to a fatal dose of the living bacteria, and yet may succumb to a fatal dose of the toxine. This has been shown to be the case with the cholera vibrio, with Metchnikoff's vibrio, and with the bacillus of hog cholera.

Indeed, it has been affirmed that immunised animals are more susceptible to the toxines than normal animals. Such a statement must be accepted with some caution, for there are many sources of fallacy. The method of preparation of the toxines may produce other compounds than are formed in the body; and due regard must be paid to the time which has elapsed since the "test" inoculation with the living cultivations. It is well known that bacteria will remain living in the body for some time after apparent recovery, and that injection with toxines will then cause a recrudescence of the disease, the animal dying of a general infection, and not of toxæmia.

Immunity may be conferred upon animals in other ways than by vaccination. Klein has shown that the previous injection of dead bacteria of various kinds into the peritoneal cavity will render animals immune to intraperitoneal inoculation with the cholera bacillus. Issaeff has extended these experiments, and has shown that the previous injection of various substances, such as broth, will produce the same effect. This form of immunity is of a different nature to that conferred by vaccination. It is of a much shorter duration, and differs also in other points.

There is still another method of producing immunity, and that is injection with the blood serum of highly immune animals. This method differs in several important points from the methods already described. Immunity conferred in this way is of short duration, while the immunity conferred by vaccination lasts a long time.

The most important difference is that the method is of therapeutic as well as of prophylactic value. Animals can be protected, even if the serum is injected some time after inoculation. By the other methods of producing immunity such a result cannot be obtained.

Having described the methods of producing immunity we must next consider certain properties possessed by the cells and humours of the body in relation to bacteria. We will first consider the process of *phagocytosis*, the ingestion

of bacteria and other foreign substances by amœboid cells; for upon this phenomenon is based one of the most seductive theories of immunity.

It is to Metchnikoff and his school to whom we are indebted for most of our knowledge of this interesting subject. He has studied the process throughout the animal kingdom, and has shown how widely spread it is. The amœba is well known to be capable of englobing and digesting foreign bodies. It will also attack living bacteria, digesting them in the same manner as it does dead particles. Metchnikoff has shown that the amœba sometimes englobes a small unicellular micro-organism which he calls the microsphæra; but it is incapable of destroying and digesting it. On the contrary, the microsphæra multiplies in the body of the amœba, and ultimately destroys it. This, according to Metchnikoff, is the counterpart of what occurs in the vertebrata. When micro-organisms are introduced into the body, the phagocytes attack and englobe them. There is then a struggle between the phagocyte and the bacterium; should the phagocyte be victorious the animal recovers, while if the bacterium is victorious the animal succumbs to the infection. To this question we shall return, but at present we must occupy ourselves only with the phenomenon.

In the higher forms of the invertebrata the phagocytes become differentiated from the other cells of the body, but still retain their power of englobing foreign bodies. The introduction of a splinter of wood into the gelatinous bell of the medusa leads to an accumulation of phagocytes, and if the object has been previously soaked in carmine, the particles of carmine are englobed by the phagocytes.

The daphnia, a minute transparent crustacean, is liable to become infested with a parasite called the *monospora bicuspidata*. The spores of the parasite gain access to the body through the alimentary canal, which they penetrate. As soon as they have passed through the wall of the intestine, phagocytes accumulate around them, englobe them, and usually destroy them. If the parasites are destroyed the daphnia recovers, but frequently this is not the case, and the parasite invades the body, and kills the daphnia.

Metchnikoff gives a large number of examples of phagocytosis among the invertebrata, and there is good evidence to believe that it is an important means of defence among these lower animals.

Let us now consider what are the cells which are

phagocytic among the vertebrata. The most important are certain kinds of leucocytes, but not all. Various classifications of leucocytes are given by different observers. Metchnikoff divides them into four varieties. The *lymphocyte* is a small cell with a large round nucleus surrounded by a small amount of protoplasm. The *mononuclear leucocyte* is a large cell with a round oval or kidney shaped nucleus, closely resembling certain endothelial cells. The *eosinophile cell* possesses a lobed nucleus, and contains in its protoplasm coarse granules staining deeply with eosine. The *neutrophile leucocyte* contains a lobed nucleus, of which the individual portions are united by delicate nuclear filaments, giving the appearance of a multinucleated cell. The protoplasm contains granules which can only be stained by a mixture of the acid and basic dyes. Hence the name *neutrophile.*

Of these leucocytes, the eosinophile cells and the lymphocytes do not possess phagocytic properties, while the mononuclear and neutrophile cells are phagocytes, and even when removed from the body are capable of englobing foreign particles such as carmine. The other important class of phagocytes are the endothelial cells of the vessel walls and of the lymphatics.

That these phagocytes are capable of englobing not only foreign particles, but also dead and living bacilli, there can be no doubt. If anthrax bacilli are injected under the skin of a pigeon, a local inflammation occurs, and if the exudation is examined it will be found to contain a number of leucocytes, many filled with anthrax bacilli. If tubercle bacilli are injected into a rabbit's vein, the bacilli quickly disappear from the blood of the general circulation, and are then found in the endothelial cells of the vessels, especially in the liver. Leprosy bacilli are frequently found within the endothelial cells of the lymphatics.

That phagocytes are capable of englobing living bacilli has been proved by several experiments. There is a form of septicæmia among frogs, caused by a motile bacterium. These bacteria have been observed to move about in the interior of phagocytes, showing that they must have been englobed in the living state.

Metchnikoff has isolated phagocytes containing anthrax bacilli, and has planted them in broth. The bacilli grew and formed a good cultivation, showing that they must have been alive when planted.

We must next consider what leads to the emigration of

phagocytes through the vessel walls, and their accumulation around the spot of inoculation. This is due to what is called *chemiotaxis*—the power possessed by various substances of attracting or repelling amœboid cells. When an attraction is exerted, we speak of positive chemiotaxis, and when repulsion, of negative chemiotaxis. It is a phenomenon which can be observed in the lowest forms of life. The myxomycetes, which consist of naked masses of protoplasm, are repelled by certain chemical substances, and attracted by others. As a rule, the phenomenon is of a protective nature, the protoplasm moving towards food, and away from harmful substances; but this is not always the case. There are many interesting facts connected with this phenomenon. The same chemical substance may attract or repel the protoplasm, according to its concentration, and the protoplasm, which is at first repelled by a chemical substance, may become gradually accustomed to it, and ultimately attracted.

In the vertebrata, chemiotaxis can be studied by inserting capillary tubes filled with various substances into the subcutaneous tissue, or into the peritoneal cavity. The tubes are removed from time to time, and examined under the microscope. If the substances introduced exert a positive chemiotaxis, the tubes are found to be filled with leucocytes, while if they exert a negative chemiotaxis, or are inert, no leucocytes are found in the tubes. The toxines produced by bacteria, especially those contained within their protoplasm, generally exert a positive chemiotaxis. Some are inert, and some are stated to possess a negative chemiotaxis. This latter property is more difficult to prove, and is denied by many observers.

Chemiotaxis will thus explain the accumulation of leucocytes around the spot of inoculation with certain bacteria, and the absence of any accumulation in other cases.

Bactericidal Properties of Blood.—A factor of great importance in connection with immunity is the power possessed by the blood and other body fluids of destroying bacteria. This was first discovered by Nuttal, and has been carefully studied by Buchner, Behring, and Nissen. It was shown by Buchner that in the case of blood the power resided in the blood serum. The best method of demonstrating this property is to inoculate the serum with a cultivation, and to estimate the number of bacteria present at different intervals by means of plate cultivation. After a few hours the number of bacteria diminish, and

ultimately all may be destroyed. But as a rule, after a time the number of bacteria begins to increase, and ultimately far surpasses the number originally present. At first the serum destroys many of the bacteria; but this power of destruction is gradually lost, and then the bacteria are able to multiply without hindrance.

It is supposed that the serum contains a substance called an *alexine*, which possesses bactericidal properties, while the bacilli secrete a substance called *lysine*, which neutralises the alexine, and thus enables the bacteria to grow. Both these substances are hypothetical, and have not been isolated. Whether they exist or not, there can be no doubt that the bacteria, if present in large quantities, can resist the bactericidal properties of the serum. Even the addition of dead bacteria to the serum annuls its bactericidal properties.

The bactericidal substances are very readily destroyed by physical agents; the exposure of serum to a temperature of 60° Cent. rapidly destroys its bactericidal property.

There is good evidence to show that the bactericidal substances are secretions formed by certain of the leucocytes. Hankin was the first to suggest that the eosinophile cells were cells which secreted alexines, and this view has been still further supported by the observations of Hardy and Kanthack. Buchner, Denys, and others have shown that inflammatory exudations containing many leucocytes possess more powerful bactericidal properties than the blood itself.

Anti-toxic Properties of Blood.—The blood of animals immunised by the method of vaccination possesses properties which are quite distinct from those of the bactericidal properties we have discussed. I am now alluding to the so-called anti-toxic property.

Behring and Kitasato proved that the blood serum of animals immunised to diphtheria and tetanus possesses the power of annulling the effect of the toxines of these diseases when injected into other animals. The potency of the serum can be enormously increased by frequent injections of the toxines in increasing doses. In the case of tetanus, a serum has been obtained which, when injected in a quantity corresponding to a ten-millionth part of the body weight, will protect an animal against an otherwise fatal dose of the toxine.

Ehrlich has shown that the blood serum of animals which have been habituated to large doses of two vegetable

poisons—ricine and abrine—possesses anti-toxic properties with regard to these poisons.

The same principle has been applied to a number of bacterial diseases, and it has been generally shown that the blood serum of immunised animals will protect other animals against the disease. Here we must distinguish between an anti-toxic serum and an anti-biotic serum. A serum which is anti-toxic is also anti-biotic. Thus a serum which will protect an animal against the toxines of diphtheria will also protect it against inoculation with the living bacillus. The converse does not hold, for it has been shown in several instances, such as in the case of hog cholera and Metchnikoff's vibrio, that a serum which will protect perfectly against inoculation with living bacilli is quite inefficacious against their toxines.

The potency of a protective serum depends upon the state of immunity of the animal furnishing the serum. A powerful protective serum can only be obtained by repeated inoculations with increasing quantities of the living bacilli or their toxines.

The bactericidal properties of a serum and its protective power are quite distinct properties, and must not be confused. The protective power of the serum is not readily destroyed by heat, and the protective substance can be precipitated in various ways without losing its efficacy. The bactericidal properties of a serum are, on the other hand, very readily destroyed.

With regard to the formation of the anti-toxic substance in the serum, there are two facts which point to the bacterial toxine entering into its composition. The potency of the serum is roughly proportional to the amount of toxine introduced into the body; and the anti-toxic properly is specific, that is to say, a diphtheria anti-toxin will only protect against diphtheria, and a tetanus anti-toxin against tetanus. There appear to be exceptions to this rule, but I think they may for the present be neglected. Now it is very unlikely that the cells of the animal body would possess the power of secreting a specific anti-toxin against every disease. We are thus led to the conclusion that the essential element in the anti-toxin serum is a derivative of the bacterial toxine, and this is also borne out by the fact that the serum is more powerful in proportion to the amount of toxine introduced.

Up to the present I have simply placed before you an account of the various factors which are concerned in immunity, without giving any opinion of their relative

importance. I must now attempt to do this. In such an attempt it is well to discuss natural immunity apart from an acquired immunity; for of the factors concerned, *i.e.*, phagocytosis, bactericidal substances, and the possession of a protective serum, the latter does not exist in naturally immune animals. There is thus an essential difference.

We will begin with the discussion of natural immunity, because the problem is a more simple one.

Natural Immunity.—At the outset, I may say that no one theory will account for all cases of natural immunity. In some cases the explanation is a simple one, and in others complex, involving several factors. Frogs are immune to inoculation with the tubercle bacillus, chiefly because the temperature of the frog is not a suitable one for the growth of the bacillus. Fowls are insusceptible to inoculation with the tetanus bacillus, because their tissues are not affected by the tetanus poison, just in the same way as they are not affected by morphia. But even these apparently simple explanations are not sufficient. We have to explain how it is that the tubercle bacilli and tetanus bacilli are destroyed after introduction into the bodies of frogs and fowls. This can only be explained by means of phagocytosis, or by means of the bactericidal properties of the fluids. Neither of these factors are sufficient to explain the destruction of the bacteria in every case of natural immunity. In some instances the bactericidal properties of the blood are quite sufficient explanation. Certain kinds of white rats are immune to anthrax, and the blood serum of such rats rapidly destroys anthrax bacilli outside the body. Any further explanation of the failure of anthrax bacilli to multiply in the body of these animals is unnecessary.

On the other hand, the same animals are but slightly susceptible to inoculation with the pneumococcus, yet their blood serum possesses no bactericidal properties in regard to this micro-organism.

In cases where the fluids of the body possess no bactericidal properties, phagocytosis must be the most important factor in immunity. Metchnikoff looks upon it as the most important factor in all cases. Some of the arguments which have been brought forward in favour of this theory are, in the light of our present knowledge, valueless. The mere fact that the bacteria are taken up by the phagocytes is no proof of immunity, for in pigeons dying of swine erysipelas marked phagocytosis occurs. Again, it has been shown by Werigo that when anthrax bacilli are injected

into the circulation of susceptible rabbits, they are at once taken up by the phagocytosis of the liver and other organs, and are again set free to multiply freely in the blood. The formation of an exudation rich in phagocytes at the spot of inoculation is also not sufficient evidence of the exclusive *rôle* of phagocytosis in the production of immunity; for such exudations contain many leucocytes which are not phagocytic, and the fluid portions of these exudations possess bactericidal properties.

I would be far from denying that phagocytosis is of no importance in connection with natural immunity; but I believe that it is rarely a sufficient cause alone and unassisted by the bactericidal properties of the fluids of the body.

In most cases of natural immunity the bacteria, when introduced, are destroyed partly by the bactericidal substances, and partly by the phagocytes. In some cases the bacteria are destroyed by the bactericidal substances alone, the phagocytes only playing the secondary *rôle* of digesting the already killed bacteria. In others it is possible that the phagocytes alone destroy the bacteria.

Acquired Immunity.—The problem of acquired immunity is a more complicated one; for in addition to the factors already discussed, we must consider the protective power of the serum. Immunity acquired to toxic diseases, such as diphtheria and tetanus, is chiefly due to the anti-toxic properties of the blood, which annuls the effect of the toxines upon the tissues. The bacteria are then destroyed by the phagocytes; for the blood serum of animals immunised to these diseases has no marked bactericidal properties. The phagocytic power of the cells was present in the animal before it was immunised, but was paralysed by the bacterial toxines. This is clearly shown in the case of tetanus by the fact that the spores, when freed from toxines, are readily destroyed by the phagocytes of susceptible animals.

In dealing with immunity acquired to septic diseases we are met with greater difficulties, which will, however, be cleared up as more facts come to light. The serum of immunised animals is protective against inoculation with living bacteria, but not against their toxines. It is anti-biotic and not anti-toxic.

In some cases, such as in that of the vibrio Metchnikowi, the immunity appears to be due to an increased bactericidal property of the blood, for the blood serum *in vitro* rapidly destroys the vibrio. In other cases the blood serum of

immunised animals has no marked bactericidal properties, and yet will protect perfectly.

Most interesting observations have been made by Pfeiffer and others with regard to infection with the cholera bacillus. When introduced into the peritoneal cavity of highly immune guinea pigs the bacilli rapidly lose their mobility and are transformed into rounded bodies. This destruction appears to be due to substances excreted by the leucocytes. The same occurs in normal guinea-pigs treated with the protective serum and subsequently inoculated.

There are many other interesting facts in connection with the immunity acquired to septic diseases, and to such diseases as the disease produced in guinea pigs by inoculation with the cholera bacillus. I feel, however, that in attempting to explain them we should only get into difficulties, and that it is only when more facts are obtained that an explanation will be forthcoming.

In conclusion, I would say that the main point which I have attempted to show in this paper is that immunity is a complex subject, and not to be explained upon any one theory. In each case we have to consider several factors; and in most cases several factors are concerned, sometimes one and sometimes another being the most essential and important.

THE INFLUENCE OF SOIL AS A FACTOR IN THE PRODUCTION OF DISEASE, ESPECIALLY IN HOT CLIMATES.

By Professor J. LANE NOTTER.

(*Read*: 17*th May*, 1895.)

The ever-varying conditions of the soil of the earth, the great changes and mysterious forces which in the lapse of ages have taken place and are now taking place within it, have occupied at all times the thoughts of man, especially with reference to the direct influence which they exercise upon our physical welfare. Even Hippocrates, in his *Trilogue*, drew attention to the influence of soil and place upon health; and made certain advices to physicians of his day on the point.

Similarly Vitruvius, Galen, Diodorus, and Thucidides made mention of the unhealthy influences of low-lying land, marshes, and water-logged soils. Although the ancients were alive to the importance of the influence which the soil exercises upon health, still the question dropped during the middle ages, and it is only a later civilisation, with new colonies to be made, that has fully recognised the fact afresh. It is quite within the last generation that what is called the geographical distribution of disease has been studied; and even by that mode of investigation, unless considered in respect to vast tracts of country and different climates, it is difficult to determine what may be the precise influences on health exerted by the soil.

If we limit the study of this question to a country so small in extent as our own, and with climatic conditions so fairly uniform, the process of eliminating other than purely local causes becomes exceedingly difficult.

For these reasons the subject is one of the greatest difficulty, and, too, one with which one observer alone can hardly efficiently grapple.

Among the most noteworthy workers at the solution of the problems involved in the great question of the influence of the soil as a factor in the production of disease, we find

Von Foder, Pettenkofer, Levy, Fleck, Von Naegeli, Schlœsing, Muntz, and Warrington, in Europe, with Lewis and Cunningham in India. The work of Pettenkofer and his pupils in Munich, coupled with the study of endemic and epidemic diseases generally, has brought to light an array of facts which very strongly suggest that an intimate association exists between the soil and the appearance and propagation of certain diseases; and which can only be explained by a series of processes taking place in the soil. Although experiments and observations made with regard to this question allow this view to be looked upon as well established, still the precise *rôle* played by the soil in an etiological respect is by no means so well understood as to make it possible to separate the single factors, and dogmatise on their effects.

The cause of the comparatively slow progress made by knowledge in this direction is due principally to the fact that the processes going on in the soil originate from a combination of various causes which in part oppose, in part support, each other; it is, therefore, with extreme difficulty that single agents can be discovered, and the effects of each one studied in separate detail.

Because of these hindrances, the scientific or hygienically correct conception of the contemplated part played by the soil in disease-production will, and can only, be attained by first considering all the facts and effects of the various isolated factors or features at work within the soil, and then regarding the results of their combined action one upon the other, and in relation to disease manifestations.

The soil itself has ever been universally regarded as in some mysterious sense the mother of us all. To us, even in this century, the manner in which soil supports the life of plants is still mysterious, for we have at the best only a limited knowledge either of the substances contained in the soil, of the changes which they undergo, or of the part they take in plant-nutrition. The soil, we know, consists of mineral, vegetable, and animal substances in varying degrees of chemical relationship, while in the interstices of its substance are air and, at times, often water; but our knowledge is largely deficient with regard to the state and conditions of the carbon, nitrogen, hydrogen, and oxygen, which constitute the organic matters of the soil. It would largely conduce to a clear understanding of the difficulties attaching to these problems if, before discussing the nfluence of soil in producing special diseases, we were able to consider the nature and extent of the general factors

which are common to all soils, and, too, which specially influence their salubrity.

Time will not, however, permit me to do so now, and I must pass on to consider the influence of soil on special diseases.

Cholera.—The earliest writers upon this disease emphasised its remarkable preference for particular places; while the history of each successive epidemic implies, besides an importation of the contagion, certain local conditions which may be either general sanitary defects or peculiarities of climate and soil.

It is now very generally accepted that the particulate contagium of cholera is the specific micro-organism, called the comma bacillus. This grows in and liquifies alkaline gelatine, but not at all in a distinctly acid medium. Its morphological and biological characters are sufficiently distinct to render its differentiation easy.

Whether it is by this particular organism alone, or whether it is only when in conjunction with some other, as yet unknown, microbe that the symptoms of cholera are generated, the general belief prevails that cholera, in this country at least, is mainly spread by means of drinking-water, though dissemination may occur in other ways, more particularly from an "excrement-sodden earth", which fouls not only water but air. On the Continent, especially in Germany, much importance has been attached to the movements of ground-water in the diffusion of cholera. This has been mainly due to the teachings of Pettenkofer, who maintained that cholera never prevails as an epidemic where soil is impermeable to water, or where the soil-water does not violently fluctuate in level. Pettenkofer admits the presence of a specific germ in the soil, which, he considers, is only able to violently manifest itself when the soil has been rendered suitable, as when the ground-water, after having risen to a higher level than usual falls again. This sequence of events is quite conceivable, by either the assumption that the sudden rise and fall in ground-water level carries into wells some organic cholera-producing matter from the soil, which otherwise could not gain access to water supplies; or by assuming that the cholera micro-organisms, if present in the upper soil-layers, is merely awakened into activity by warmth and moisture, and subsequently becomes diffused into the atmosphere, as a drying zone of soil forms on the fall of the soil-water.

The latest utterances of Pettenkofer emphasise this

latter view, for he says, the rise and fall of the ground water are but an "index of the humidity or moisture of the porous and permeable soil which overlies the ground water". Fodor, at Buda-Pesth, has demonstrated an association of soil moisture and heat as indicated by the fluctuations in the ground water and rise in temperature with cholera prevalence. Pfeiffer has also noted a direct relation between soil temperature at a depth of 3 ft. to 6 ft., associated with soil moisture and the prevalence of cholera epidemics.

It has been alleged (for India, at least) that no widespread epidemic of cholera can occur unless during or after rain. This can be readily understood if we assume, with Pettenkofer, that soil moisture, as distinguished from absolute dryness or saturation, heat, aeration, the spread of the specific germ and filth, are the essential earth conditions for the spread of cholera. On the other hand, rainfall, sufficient to saturate the soil, will tend to arrest the disease, however high the temperature may be, owing chiefly to the micro-organisms being carried further from the surface, where they are no longer among favourable surroundings. If rain merely moistens a previously foul and dry soil, the other conditions being present, it may induce an outbreak. Given a moist soil, prolonged heat and drought may establish conditions most conducive to cholera.

It must not be overlooked that unless these various agreements between cholera curves and curves of soil heat, moisture and ground water are to be regarded as mere coincidences, an essential factor to explain their association with cholera prevalence is the presence in the soil itself of the specific germ. Assuming this to be the vibrio known as the comma bacillus, it is interesting to find that in no cholera epidemic has this micro-organism been found in, or isolated from, the soil; though cholera commas have been repeatedly demonstrated to be present in the sand placed in filters in India. To those familiar with the countless numbers of bacteria present in even comparatively clean soils, and the difficulties experienced in obtaining pure fractional cultures of particular forms from impure growths, this non-isolation from and failure to find in soil samples of the cholera vibrio will not be surprising. Though this micro-organism has not been found in soil, many observations have been made regarding its behaviour and fate when introduced into soil samples. Experimental facts indicate that choleraic comma bacilli are, under

ordinary circumstances, somewhat feeble in the struggle for existence; and when introduced into soil and water of varying qualities, so long as these retain their natural conditions, tend to disappear, mainly owing to the influence exerted on the commas by other fungi and schizomycete organisms. Cunningham's experiments, made with garden humus kept moist under a bell jar, show a survival of cholera commas for some forty days. If earth were much fouled, as by a mixture of fæces, the commas were not recoverable later than five to nine days; if the fæces, before mixture with the soil, were boiled, the commas were found as late as the twenty-sixth day. Dempster's experiments indicate that the degree of moisture is a factor of the greatest importance in regard to the retention of vitality of these organisms in soil, and this may be the explanation in part of the endemic and epidemic prevalence of cholera. In Lower Bengal the soil is always moist, and cholera is endemic, but is lessened during the heavy rains, when the soil becomes saturated; in the Punjab the soil is dry, and epidemics do not occur unless some amount of rain has fallen: in the one case the rains hinder, in the other they favour the appearance of cholera. The difficulty which comma bacilli appear to have in surviving in such media as earth or water, appears to be mainly due to their inability to form spores or otherwise assume a resisting form. It is necessary, in this connection, however, to remember that among the many comma bacilli obtainable from cholera dejecta, there is, in all likelihood, a plurality of species which do not behave uniformly in water soil or other media. By a due appreciation of this fact, it is probable that many experimental inconsistencies may be explained.

The general evidence indicates that the specific bacteria of cholera discharges are capable of a much longer existence in the superficial soil layers than has hitherto been supposed; and, consequently, it is specially necessary to guard against pollution of the soil, and, through it, against the probable contamination of both water and air. In India, all evidence points to the soil as playing a very large part in the diffusion of cholera, chiefly as affording a nidus in which the comma bacilli can retain their vitality, if not actually multiply, for long periods. The soils in which this sequence of events seems particularly to occur are the loose and partially moist sands in the beds of rivers and along the sides of tanks and other bodies of water used for drinking and laundry purposes. While the connection between soil conditions and cholera prevalence appears to

be true for some localities, particularly its areas of endemic prevalence, the evidence is not sufficiently strong to warrant its universal application; in fact, the diffusion of the disease is largely dependent upon other factors than soil states.

Diphtheria.—All accounts of diphtheria show a tendency on the part of this disease to recur in the same districts year after year. The question naturally suggests itself, are the reappearances due to a revival of the contagion derived from previous outbreaks in the same place, or to some favouring condition which the place offers for the development of infection derived from some other quarter; and have these favouring conditions any dependence upon the character and state of the soil? As far back as 1858 Dr. Greenhow stated that diphtheria was especially prevalent on cold, wet soils. Later, in 1881, Dr. Airy describes the localities affected as, "for the most part, cold, wet, clay lands", but he adds, "there is evidently great variety in the soils on which diphtheria can prevail", for it is found in full force on the chalk downs of Kent, on the loamy sands and clays of the Sussex Weald, on the alluvium and boulder clays of Essex, on the marls of the new red sandstone, and on the slopes of the slate-rocks of Wales.

An analysis of the innumerable outbreaks of diphtheria in various parts of Europe, indicates that the geological features of the affected districts appear to play a less important part in the incidence of the disease than does soil dampness. This is especially well shown by Kelly and Barnes, in their accounts of the epidemics occurring in Sussex and the Eastern Counties respectively. The latter shows that in five parishes, comprising 1,813 inhabitants, on a dry gravelly soil, only one outbreak of diphtheria occurred in eleven years; whilst in twenty-seven other parishes, with 14,000 living on a subsoil of clay having a percentage of water in it as high as 90 per cent., no less than forty-eight outbreaks occurred in the same period. These figures show the relative proportion of outbreaks to inhabitants as being 1 in 1,800 on dry soil, and 1 in 300 on wet.

A very interesting series of facts, dealing with the inter-relationship between diphtheria-prevalence at Maidstone and movements of the subsoil-water, are given by Dr. Adams.

His observations, extending over nine years, show that a strict concordance may be traced between soil-dampness

and diphtheria on the one hand, and absence of diphtheria and soil-dryness on the other. But the dampness and dryness of the soil depend upon the rise and fall of the ground-water, and have their appropriate seasons; so long as the order of this occurrence is preserved, health is maintained. As long as the soil is well washed by the winter's high tide, and afterwards dried and aerated during the summer's low tide, all goes well; but so soon as these salutary movements are arrested, or their order disturbed, diphtheria prevails, reaching its acme of prevalence when stagnation at a relatively high level is most complete.

For the conception of this relationship between the movements of the subsoil-water and the prevalence of diphtheria, it is assumed that the germ of the disorder resides in or upon the soil, and is liable to be displaced and dispersed along with the subsoil air. Adams maintains that the two chief agencies concerned in the discharge of the soil-air into the atmosphere we breath are reduction of atmospheric pressure, which acts by aspiration, and rainfall, which operates by compression.

Probably the latter is by far the most effectual, though both may often act in concert.

In connection with the foregoing generalisations, it is interesting to note the actual behaviour of the diphtheritic contagium in soil. We have, indeed, no actual proof that the bacillus is either an ordinary or an occasional resident of the soil, or that it becomes air-borne in sewer-gas or soil emanations. Experiments show that pure cultures of this bacillus, when mixed with garden-soil constantly moistened short of saturation, and kept in the dark at a temperature of 14° C. will retain their vitality for more than ten months. They die out from moist soil kept at 26° C., in about two months; from moist soil at 30° C., in seventeen days; and in dry soil, at the same temperature, within the week. In the laboratory, absolute soil-dryness is as distinctly antagonistic to the vitality of the diphtheria bacillus as soil-dampness is favourable.

These experimental results explain to a large degree the general absence of diphtheria throughout the plains of India, and its endemic prevalence in the Indian hill-stations, and Europe generally.

The peculiar connection of subsoil-water levels with diphtheria prevalence, as emphasised by the experience at Maidstone, is to be largely explained by the influence which they have upon the greater or less degree of soil-dampness.

Both statistically and experimentally, we find that a damp soil favours the life and development of the diphtheria bacillus, while prolonged submersion and drought kill it. In the incidence of epidemic diphtheria we are justified in regarding, in country places at least, constant soil-moisture as a chief factor; while, possibly, in the case of urban outbreaks, mere soil moisture is subsidiary to other more potent causes.

Enteric Fever.—There is much in the mode of its occurrence and diffusion which points to enteric fever being intimately bound up in its origin to conditions of locality. It is rare to find endemics or epidemics spreading over any considerable tract of country. Such outbreaks are nearly always confined within local limits; and are, in every sense of the term, local epidemics; their exacerbations being absolutely local also.

The local character of the disease is very prominent in cases where its occurrence is frequently confined to single quarters of a town, a single street, or group of houses. The development of the disease is so bound up with definite conditions of the place or area affected, as to suggest that certain hygienic circumstances of their localities, or certain factors situated in the soil itself, must furnish not only material for its origin, but also for the continuance of typhoid sickness.

Observations made at the most diverse parts of the globe, and the general distribution area of the disease, show that mere questions of elevation, or even configuration of the ground, have little or no influence on the occurrence of the affection. On the other hand, the same observations go to show that the disease is met with oftener on the more recent formations than the older, which, so far as concerns the physical characters of the soil, is identical with the questions of permeability to air and water. Magne, quoted by Hirsh, made inquiries into this matter, using as his material the epidemiological reports sent in to the Paris Academy of Medicine from 1841 to 1863, omitting 1858: he came to the following conclusions. Of the 757 specimens reported, 564 occurred on the more recent formations, sixty-four on primary or transition rocks, while the remainder were on soils belonging partly to the older and partly to the more recent formations.

Pettenkofer's observations on the wells at Munich led Buhl to the discovery that in that city there is a very close relation between the height of the ground-water and the

prevalence of enteric fever: the outbreaks of enteric fever occurring when the soil-water was lowest, and especially when, after having risen to an abnormal height, it had rapidly fallen. The observations have been further extended by Pettenkofer with the same results. The point has also been investigated by Leidel in Munich and Leipzig for the years 1856-64, and 1865-73, and, from a mathematical consideration of the numbers, he concludes that, according to the theory of probabilities, it is 36,000 to 1 that there is a connection between the two occurrences.

Ranke, on the other hand, states that no enteric fever exists in the neighbourhood of Munich, but what is imported from Munich itself, although both the soil and ground water are the same. Munich has a soil consisting of sand, with a peculiar power of holding nitrogenous substances: it is largely honeycombed with cesspits, from which more than 90 per cent. soak into the surrounding soil, and, as the streets are well paved, the houses of the town constitute the only outlets for the foul soil air. A very similar argument, together with some very interesting facts concerning the prevalence in Dublin of enteric fever, has been brought forward by Sir C. A. Cameron. For some years a persistent occurrence of this disease has existed in Dublin, which cannot be accounted for either by polluted water, milk, or food, and which has not very sensibly decreased even after an improvement in the water supply. Sir C. A. Cameron attributes this prevalence to the practice, which has been in use in Dublin for years, of storing excreta in pits, so that the soil has become thoroughly saturated with the specific micro-organisms of the disease; these, he thinks, are carried into the atmosphere by displacements of ground air. According to him, the ratio of cases to population living in Dublin on loose porous gravel soil, for the ten years 1881-91, was 1 in 94; while the ratio for those living on stiff clay was but 1 in 145. "This is what we should expect, since the movements of the ground air are much greater in loose porous than in stiff clay soils. No pronounced relation has been found between the death-rate or prevalence of enteric fever and the temperature or putrefactive activity of the soil.

Assuming that there is some connection between oscillations of the ground water or movements of the ground air and enteric fever, it is necessary to realise the existence within the soil of the specific germ. This is now accepted as being the Eberth-Gaffky bacillus. No experiments have

as yet demonstrated the presence of this particular microbe in soil, under ordinary circumstances; but many observations have been made which show the possibility of this micro-organism existing for considerable periods of time under certain conditions of warmth and moisture.

Dempster's experiments show, working on a dry soil where evaporation was allowed to take place, that in white crystal sand, the bacilli can be found, after mixture, up to the ninth day; in yellow sand up to the eighteenth day; and in garden earth up to the fourteenth day. On the moist soils, on the other hand, the following were the results: in moist white crystal sand the enteric bacilli were alive on the twenty-third day, in yellow sand and in garden earth on the forty-second day. Experiments made with peat show that on this soil these bacilli do not survive longer than twenty-four hours.

Observations made with various soils brought from the tropics and belonging to the loose sandy marls, show that, under conditions of medium moisture, enteric bacilli can live in such soil about five weeks, but that when the soil was allowed to become dry the bacilli could not be found after the third week.

Although a vast amount of evidence has accumulated, which indicates the specific contamination of food and drink as the most frequent channel of infection for enteric fever, still it must not be overlooked that dried enteric excreta, particularly when superficially buried in dry sandy soil, may be carried by air and distributed as dust upon food.

This is a point of importance in connection with the maintenance and management of the dry earth-closet system in hot countries, like India, where possibly it is not an infrequent way in which enteric fever is spread.

Notwithstanding the discordance of the facts noted from various localities in reference to the connection between the action of either ground water, or air, and enteric fever prevalence, a few authentic cases taken in conjunction with the results of laboratory experiments, which show the possibility of the specific bacilli being able to remain alive some time in earth, it is difficult to deny the importance of the soil as a possible breeding-place of the enteric germ.

Diarrhœa.—In especial relation to that peculiar form of diarrhœa which is apt to prevail epidemically in summer and autumn, a considerable amount of evidence has been brought forward of late years to associate its connection with life processes of micro-organisms present in the superficial soil layers, but as yet not satisfactorily isolated.

It is of very general knowledge that diarrhœal mortality is low in places built on solid rock, but high where the soil is porous and loose, also upon sand or a rich surface mould. Gravel or coarse sand varies in its relation to the disease mortality and prevalence in proportion as the loose elements or stones vary. The more gravel approaches to sand in its fineness or to rock in its coarseness, so its relation to diarrhœa appears to be greater or less. Clay soils do not appear in themselves to be specially favourable to diarrhœal prevalence or mortality. The marls are either favourable or unfavourable to diarrhœal prevalence in proportion as they are loose and permeable on the one hand, or plastic and stiff on the other.

Ballard, who was one of the first to indicate any possible connection between soil states and diarrhœa in this country, thinks that the presence of much organic pollution renders a soil distinctly more favourable to a high diarrhœal mortality than it might otherwise be. Such organic fouling need not be of fæcal or excremental nature. For these reasons, diarrhœal mortality and prevalence are apt to be high where dwellings are built upon made ground, upon the refuse of towns, upon reclaimed areas, or upon the sites of old market gardens, and in places where the earth beneath and around is polluted by collections of liquid filth in cesspits, or where sewage has soaked into it from the surface of the ground.

It is the opportunities for the collection of organic filth in the fissures of certain kinds of rocks that seem to impart to them, when built upon, a diarrhœal character.

In discussing the influence of moisture of a soil, Ballard remarks, that the successive wetness and complete dryness of soil appear to be both unfavourable to diarrhœal prevalence.

The degree of habitual moisture, specially favourable is that amount which, while being marked, is not sufficient to preclude the free admission of air between the constituent physical elements of the soil. Such a degree of dampness occurs when the subsoil water stands sufficiently near the surface to maintain, by capillary attraction, the dampness brought about by previously greater nearness of the water to the surface; or when the soil, as in the case of marls, contains sufficient of the clayey element to imprison some of the water saturating it at some time previously. The requisite degree of soil dampness may be produced by floods or from habitual surface soakage. One of the most important conditions indicated by Ballard, as influencing

the prevalence of diarrhœa in England, was the temperature of the soil. As the result of many years of observation, regarding the relationship between diarrhœal prevalence and the earth temperature at depths of 1 ft. and 4 ft. from the surface, he says:—"The summer rise of diarrhœal mortality does not commence until the mean temperature recorded by the 4 ft. earth thermometer has attained somewhere about 56° Fahr., no matter what may have been the temperature previously attained by the atmosphere, or recorded by the 1 ft. earth thermometer." The maximum mortality from diarrhœa appears not to occur until quite a week after the 4 ft. thermometer attains its maximum mean and declines gradually with the decline of the temperature recorded by the same instrument. The heat of the atmosphere and of the more superficial soil layers appears to exert but a subsidiary influence upon diarrhœal prevalence. Very similar results were obtained by the late Dr. Tomkins, and more recently by Dr. Priestley at Leicester.

Speaking generally, both Ballard and Tomkins express a belief that epidemic diarrhœa, as observed in England, is due to a soil-bred organism, which, at times, escaping from the earth, becomes air-borne, and thence gains access to the human body by food or drink, probably by milk. This organism has not been isolated so far, neither is there any definite evidence forthcoming from either the continent or the tropics which causally connects epidemic diarrhœa with soil conditions.

We are only too well aware that fermenting and decomposing food, especially milk, may cause diarrhœa; and since these processes are mainly the result of bacterial action, these latter may be regarded as the fundamental causes of the disease. But it is open to doubt whether sufficient evidence exists to show that these organisms ordinarily reside in, or are at all dependent upon conditions of, the soil, to permit our forming a working hypothesis upon these lines.

Malaria.—Although no definite relation has been shown to exist between the carbon dioxide in soil-air and malaria, rainfall, on the other hand, appears to bear a direct relationship to the disease, the malarial curve generally falling during rain, but generally rising as dry weather sets in. The differences of density in the soil-air appear to bring about an escape of the malarial miasm with the atmosphere, rather than do currents of atmospheric air by aspirating the soil. It is owing to its rarity, as compared

with that of the air above, that the ground-air tends to rise into the atmosphere towards evening; and it is after sundown, when the atmosphere is very generally polluted by ground-air, that malarial infection most often occurs. These facts suggest the improbability of the malarial germ being present in the dust on the Earth's surface, but rather that it is contained in the ground-air below the surface; the frequency of malarial infection following digging up of soil, largely confirms this.

There is little evidence to show that malaria is caused by any mineral constituents of the ground, more particularly as it prevails upon soils of such widely different composition as alluvial, sandy, or ferruginous earths as well as upon soils formed from the weathering of metamorphic rocks.

If we accept the microbean origin of the disease, a malarious soil will depend not on the nature of its organic constituents, but upon the presence of those conditions which favour the life of the specific organisms, and bring about their aerial diffusion. These organisms are probably allied to the flagellate protozoa, and have been described by Laveran, Marchiafava, Celli, Richards, and Oster, as the *plasmodium malariæ*. It is an amœboid-like body which appears to separate out the pigment of the red blood corpuscles. Its spores, which eventually form, are only found in the spleen, and give rise to fresh amœba. These pass into the blood, causing renewed fever, the nature of the attack being determined by the rapidity with which the different stages take place. So far, this micro-organism has not been found in the soil, nor, indeed, has it ever been cultivated outside the human body.

Malignant Œdema.—This is a fatal disease of mice, guinea-pigs, and rabbits. In man it is commonly spoken of as progressive gangrenous emphysema, being chiefly observed after compound fractures or wounds, and characterised by a rapidly-spreading œdema in which the subcutaneous tissues, commencing at or near the injury, get distended with a clear reddish fluid containing bubbles of gas.

The frequency of this affection following injuries or wounds into which earth had passed or come into contact, early drew attention to the connection between it and soil. Its infective agent is now known to be a bacillus morphologically similar to that of anthrax, but differing from it in being anaerobic. The œdema bacillus appears to be widely distributed, being found in the most varied putrid

substances—in the bodies of decomposing animals, in fæces, and in every specimen of earth which has been impregnated with putrifying matter. These bacilli are particularly found in garden earth, after recent manuring, where they appear to be able to pass through their characteristic cycle of development as saprophytes.

Phthisis.—In some way, which is not clear, a moist soil produces an unfavourable effect on the lungs; at least in a number of English towns which have been sewered, and in which the ground has been rendered much drier, Buchanan showed that there had been a diminution of deaths from phthisis. Bowditch of Boston, U.S.A., and Middleton of Salisbury, noticed the same fact some years previously. Buchanan's evidence is very strong as to the fact of the connection, but the nature of the link between the two conditions of drying of soil and lessening of certain pulmonary diseases is unknown. The importance of his observations was so great that he was directed by the Privy Council to make a special investigation in the three south-eastern counties—Surrey, Kent and Sussex—for the purpose of determining whether any relation could be traced between the prevalence of consumption and the state of the soil as regards moisture. In instituting this comparison, Buchanan classified the several districts as having mainly soils permeable by moisture, or soils of such a character that water was unable to escape from them, so that they might be called retentive.

An exact comparison, however, between retentive and permeable soils, in regard to the prevalence of phthisis, was afforded by a limited area, the Wealden, which in part is formed by the weald clay, in part by the Hastings beds of alternate sand and clays. There are, indeed, no districts wholly of sand to contrast with others wholly of clay; but there are great differences in the proportion of the two soils in different districts. Hastings beds, with a population of 95 per cent. on sand and 5 per cent. on clay, giving the highest death-rate from phthisis, and Petworth on the lower green sands, having a population of 30 per cent. and 70 per cent. on weald clays without gravel, giving the lowest.

On the other hand, Dr. C. Kelly has subjected these conclusions to much criticism. He points out that most of the impervious beds are to the north of the South Downs, and that consumption seems most common in places which are bleak and exposed to damp. He insists on the fact that in West Sussex (as indeed throughout England

and Wales) there has been of late years a great decrease in the mortality from consumption, although there has been no change in the drainage in Sussex. Kelly is inclined to attribute it mainly to the progress which has taken place in the social state of the rural population. These more recent inquiries, when contrasted with Buchanan's earlier ones for the same localities, do not so much indicate the earlier conclusions to be wrong, as that all varieties of soil being now equally healthy, the cause of phthisis which still occurs have to be sought for in other conditions than soil-dampness.

It is probable that dampness is merely one of the many factors which are concerned in causing a predisposition to phthisis. On low-lying damp soils colds and catarrhs are notoriously more common than on high and dry situations, and the tubercle bacillus, which is the exciting cause of phthisis, finds a favourable nidus in these cases. We have no reason to think that soil in any particular condition affords a more favourable medium for the prevention of the bacillus than do other materials. Even admitting that soil-dampness may favour the prevalence of phthisis by tending to lessen the existence of the individual to the specific bacillus, it is obvious that many other conditions, such as overcrowding, poverty, ill-feeding of children, may all equally exercise a powerful influence on phthisis mortality.

Rheumatism.—In respect of soil-influences it is necessary to make a distinction between chronic rheumatism and acute rheumatism or rheumatic fever. It is well known that the chronic rheumatic diseases prevail most in deep and damp valleys, along sea-coasts, the shores of rivers, and in places which are much exposed to wind. Whatever connection chronic rheumatism and allied affections have to soil states, is probably in so far as altitude, configuration, and physical characters of the soil affect the climate of the particular place.

Of all soil-conditions, dampness is that which will be most likely to predispose to chronic rheumatism, because it makes a locality cold; and this is particularly likely to occur on clays in low-lying districts. Beyond this general statement we cannot go.

As relates to acute rheumatism or rheumatic fever, the facts are not so simple. The tendency of modern thought is to regard rheumatic fever as a specific febrile disease dependent upon a specific micro-organism.

A study of its epidemic prevalence shows that, at

intervals of a few years, rheumatic fever tends to prevail epidemically.

These epidemics occur in or just following years of sparse rainfall. This produces its effect by its influence in causing a warm and dry subsoil, usually with an exceptionally low ground-water. From this point of view rheumatic fever is essentially a soil-disease, having a close relationship with erysipelas and other septicæmic diseases. The explanation of the epidemic prevalence of rheumatic fever, as well as erysipelas and puerperal fever, lay in the favouring influence of a dry and warm subsoil on the specific contagia of these diseases.

Whether these contagia were alternately parasitic and saprophytic, or each case implied a fresh infection from the soil, is still doubtful. It is noticeable that the conditions of soil producing rheumatic fever and chronic rheumatism appear to be almost exactly opposite to one another. The seasonal distribution of the two diseases is also dissimilar.

Yellow Fever.—The whole history of yellow fever goes to show that though at one time and another its diffusion has been wide, still its native habitat is much less extensive. The extent of its diffusion indicates an independence of geographical origin of soil. On the other hand, the prevalence of the disease along the banks of tropical rivers which are dry at certain periods, and in the lower parts of tropical sea-ports, particularly those abutting on, or overhanging harbours, stagnant waters, or foul fore-shores, clearly defines the existence of a porous, loose, and periodically saturated soil as being a constant concomitant of yellow fever.

No evidence appears to be available regarding the microscopic life present in soils of these kinds, neither are there any data which show that soil-temperature taken alone has any bearing upon the occurrence of this peculiar disease in its endemic home. Although we do not know exactly what is the infective agent of yellow fever, yet, judging from its analogy to other infective diseases, such agent is in all likelihood a micro-organism, and that this microbe is a soil-resident is very suggestive from facts noted there.

The philosophical and remarkable theory regarding the peculiar origin of this disease, as elaborated by Creighton from the forgotten writings of Andouard, has a direct interest and bearing upon the connection between the affection and soil processes. The Andouard-Creighton

theory of the evolution of yellow fever, is that its advent into the world coincided with the rise of the slave trade, and that its habitat has been, and is, the ports of debarkation of these slaves. It is perfectly well known, from the writings of both Lind and Bancroft, that the slaves on the slaveships did not suffer from yellow fever, but much from dysentery. According to Andouard, yellow fever originated at all its endemic centres from the filth of the slave-ships, which filth was the putrid dysenteric discharges of the sick negroes.

Regarded in this light, yellow fever has been given us in the dejecta of another race, which, brought in considerable quantities in the bilges of ships to ports, has then been discharged into harbour-mud and soil. The scourings of these ships, fermenting and multiplying in the harbour and shore-mud, has generated a specifically poisonous virus which has only too readily been carried from harbour to harbour. In connection with this view, it is curious to note the fact that yellow fever is most persistent at places where there has been least cleansing of harbours, beach, or foreshore, by the natural action of the tides, or where much stagnation of the harbour-water exists, as in places like Havana, Port Royal, Bridgetown, and Port au Prince.

Though, up to the present, the isolation and identity of the yellow fever poison has not been made, still there is much to make us regard it as being essentially a mud or soil-contained poison, and the mud or soil where it has accumulated for a time, continues to be an endemic focus of the disease. Until we know more as to what is the nature of the yellow fever virus, we must leave it an open question as to how far the soil acts as only a nidus for it, or how far it exercises any specific influence on its growth or spread.

As in the case of the other infectious diseases, it is probable that the soil only furnishes a suitable medium in which the yellow fever poison may remain and multiply, and that, of itself the soil is quite devoid of any special vitalising influence upon the cause of the disease.

OBITUARY.

Sir George Buchanan, M.D., LL.D., F.R.S.

George Buchanan was born in the year 1831 in Myddleton Square, Islington, where his father was in medical practice. He received his early education at University College School, and subsequently at the adjacent college. During this period he worked under T. H. Key and de Morgan; he acquired much fondness for classics, notably the English classics, and he was awarded medals in biology and chemistry. In 1851 he took the B.A. degree at the University of London. After this he entered University College Hospital, where he won medals and scholarships in Anatomy, Physiology, Medicine, and Surgery; and in 1854 he took his M.B. degree, becoming M.D.Lond. in the following year. During this period he retained his connection and interest in University College, of which he was made a Fellow, and subsequently a member of Council. He also took an active part in the affairs of the University of London, and helped in obtaining the representation of the graduates on the governing body by means of Convocation. He was amongst the first graduates to be elected by that body to the Senate, and he was foremost amongst those who secured the admission of women to the classes in University College, and to the degrees of the University of London.

That career with which his name and life are so prominently identified was from the first influenced by the line of work he took up. On leaving University College Hospital he became Resident Medical Officer, and later on Assistant Physician to the London Fever Hospital, where he was constantly associated with the late Dr. Murchison and with Sir William Jenner. He was early impressed with the need for separating the different infectious fevers then received into the London Fever Hospital, an end which was not finally achieved until 1860; and later on he wrote the article on "Typhus" in Reynolds' *System of Medicine.*

In 1858 he became a Member of the Royal College of

Physicians, his practice in Gower Street being essentially that of a physician. He also held, in addition to his appointment at the London Fever Hospital, the post of Assistant Physician to the Hospital for Sick Children; the experience gained at the latter hospital forming the basis of his Lettsomian Lecture in 1867. Gradually attracted to public health work, he became Medical Officer of Health to St. Giles' in 1857. Here he found a field for work which was practically untrodden. The district was notorious for its sanitary evils and shortcomings; for its haunts, crowded with the lowest population and criminals; and for having a death-rate one-fifth greater than that for the metropolis as a whole.

His reports on St. Giles' from 1857 downwards form a type of what such reports should be. The earliest one shows that he studied the physical and natural characteristics of his district; its soil, drainage, and water-supply; its social peculiarities, and the density of its population; the rate in the increase of its population, its death-rates, infantile and other; its trades, common lodging-houses, slaughtering establishments, etc. He next divided his district into separate areas, stating for each one the evidences it afforded in favour of health or disease, as compared with the other areas. And, finally, he made an exhaustive statistical examination of the mortality records, demonstrating for the several areas the particular causes of excessive and preventable mortalities, and indicating how far these were brought about by undue aggregation of houses on area and of population in houses. Many of his later reports show the incessant labour devoted to the improvement of public health in St. Giles'; and a certain number of them retain to this day a foremost place in the standard public health literature of this country. In one he takes up the question of uncertified deaths; in another the regulation of bake-houses and cow-sheds; in a third he deals with the question of the isolation of the infectious fevers; in a fourth with cellar dwellings and town cemeteries, and in yet another with cholera. Gradually he was expounding some of the highest principles of public health and epidemiology. In his 1860 report, for example, is a formulation of the law of distribution of disease in an exceptional year:—"Given, a number of localities habitually affected to different degrees by diseases of a certain class. Let these diseases occur with exceptional prevalence, either in the way of increase or subsidence. Then it is those localities which habitually suffer *most* from such diseases

that are chiefly affected by their *exceptional* incidences in whichever direction the exception may be."

Perusal of these reports alone suffices to explain the reason which induced Sir John Simon, then Medical Officer to the Privy Council, to seek in George Buchanan a helper in his work. His services to the Government began in 1861, and a review of them may be given in the words of Sir J. Simon in his work on *English Sanitary Institutions*, p. 393:—

"Dr. Buchanan had been associated with the Medical Department under the Privy Council, first as frequent occasional inspector, then (1869) as permanent inspector, and soon afterwards as assistant medical officer; always rendering the very best service which the occasion required or permitted; and in various cases the author of reports which have become classical in sanitary literature. Of thorough training and habit in all ordinary relations of practical medicine, highly informed in the sciences which assist it, and of sanitary experience such as only of late years has been possible to any man, and in his case many times larger and more various than almost any of his contemporaries could have had, Dr. Buchanan had always shown himself of extraordinarily active and discriminating mind, and always intent on that exactitude which is essential to scientific veracity, whether in observation of facts, or in argument on them. He, too, had the advantage of apprenticeship in schools of science nearly twenty years more modern than those in which his two predecessors had learnt. Devoted to his branch of the public service, and with every intellectual qualification for excelling in it, he equally had the rectitude and unselfishness of character, without which there can be no official leadership in such duties."

Some of Dr. Buchanan's most valued services to the State were rendered during the years 1861-68, prior to his permanent appointment as an Inspector in the Medical Department of the Privy Council Office. It was during the earlier years of this period that he conducted systematic inquiries into the local working of the then vaccination laws, with results embodied in the amending Act of 1867. He also, by his personal association with the authorities concerned, was able to secure the prevention and limitation of epidemic typhus in the cotton districts of England in 1862; and in 1865 he undertook and fully reported upon his memorable investigation of the matter of works of drainage and water-supply, and lowered mortality rates, as cause and effect, in a number of selected districts. It was as a result of this important inquiry that Dr. Buchanan was detailed for further and special study of the apparent association of phthisis and dampness of soil, which had for its conclusion demonstration of an actual relation between the one and the other.

About the time of the transference of the Medical Department of the Privy Council Office to the Local Government

Board, Dr. Buchanan was appointed to the post of Assistant Medical Officer, and on December 31st, 1879, he became principal Medical Officer to the Board. In 1864 Dr. Buchanan was made a Fellow of the Royal College of Physicians of London, and in 1881 he became President of the Epidemiological Society, an occasion on which he delivered his inaugural address on "Aids to Epidemiological Knowledge." In 1882 he received the blue ribbon of British Science, being made a Fellow of the Royal Society. At this date he was actively engaged as Medical Officer to the Local Government Board; and by means of his work in the Public Health Department of the State he had acquired a high reputation in both hemispheres. During his tenure of that office he presented seventeen volumes to the Board, embodying the main work of the Department over which he so ably presided; and on his retirement in April, 1892, he received the honour of Knighthood. In his administration of the Medical Department, Dr. Buchanan brought to bear all those qualities, which he so richly possessed, for efficient organisation and grasp of difficult problems. During the period of European cholera prevalence in 1884-86, he organised a special sanitary survey of coast and inland districts, and thus led to greater activity in matters sanitary, especially along the English littoral, as evidenced in the period of cholera threatenings of the past few years. In this and in other ways Dr. Buchanan was ever on the alert to safeguard the public health, while devoting himself entirely to the commonweal. No less assiduous was he in the administration of the grant annually voted by Parliament for the purposes of scientific research, as his annual reports amply testify; our increase of pathological and chemical knowledge available for the repression of disease bearing witness to the usefulness of these scientific investigations. As head of a department he won not only the esteem but the affection of all who worked under him; indeed, his entirely unselfish disposition made him forgetful of his own interests, whilst he was always alive to those of his colleagues.

His retirement was brought about by slowly failing health, and the knowledge that the term of his life would be only short. But he continued at work, occupying himself actively with the concerns and interests of the London University, where he had a seat on the Senate, and of the Apothecaries Society. It was also about this date that he was made Honorary LL.D. of the University of

Edinburgh, and that he was asked to serve as a member of the Royal Commission on Tuberculosis. The latter post he willingly accepted; and, as is well known, he later on undertook the heavy task of compiling the report of the Commission, he having been made Chairman on the death of Lord Basing.

Two little incidents, which are eminently characteristic of George Buchanan, may well close this memoir of one who won the respect and esteem of all who know anything of the motives which influenced him in the several duties which he undertook. Whilst studying at the London Fever Hospital he entered for the honours examination for the degree of M.B. in the University of London. He was very ill at the time, but determined to proceed to the end. On leaving the examination hall on the last day he went straight to his bed, where he lay for a long time suffering from a severe attack of typhus. His honours examination, which ended in his receiving the Gold Medal of the University, had been carried out whilst, unknown to himself, he was passing through the first stage of a disease which nearly cost him his life. The second incident relates to his acceptance of the Chairmanship of the Royal Commission on Tuberculosis. He was again very ill, and he knew that an operation, to which he had decided to submit, was the only means of adding some little extension to his life. When urged to accept the post, he consulted his medical advisers, and being told that, so far as could be judged, a further short delay would not materially affect his future prospects, he at once undertook the laborious duty in question. Telling a friend, a few days later, of his decision in this matter, he explained that though his sufferings were considerable at the time, yet having convinced himself that the interests of his family were not likely to suffer through any shortening of his life by postponement of the operation, he was very desirous to serve his Queen to the last, and that this motive had governed his action in the matter.

Within a few days of the presentation of the report of the Royal Commission, he placed himself in the hands of his trusted advisers, the operation being most successfully performed. But, convalescence being already well established, he succumbed on the morning of May 5th, 1895, owing to sudden failure of the heart's action.

Walter Dickson, M.D., R.N.

It was on the 9th November 1894 that Dr. Walter Dickson, who held the Presidential Chair of the Epidemiological Society of London during the years 1885-1887, passed away from among us, leaving deep footprints on the sands of time and much regret to those who knew and valued him.

Born in Edinburgh on the 1st February 1821, he was educated at the High School of that city. It was at the University of Edinburgh that he received his medical education. His early youth gave promise of his future; for his assiduity, his love of work, during this period of his life, were such as to augur well for that future. He graduated in 1841 with first-class honours; his Thesis on "The Pathology of Dropsy" being the third in order of merit.

His choice of the Royal Naval Service as a career, in preference to any other, arose from association, linked with a spirit of adventure and enterprise; for having a paternal uncle in that service and believing it to be a soil of hopeful promise, he made a choice of it and embraced it.

He entered the Naval Service in the month of August 1841, and served as Assistant-Surgeon from that period to the year 1849, on Home, Cape of Good Hope, and Mediterranean stations; and as Surgeon he served on West Coast of Africa, North Sea, North-American, and West Indian stations, and East Indian and China stations.

The services he rendered during this period of his Naval career were marked with a sense of duty and of fulfilment which won for him the approval of the authorities, and the respect and esteem of those with whom he came in contact and with whom he served.

In the Naval operations in China in 1859; in the Antarctic Expedition of 1844-45, of which he has left a pleasing and interesting account; on the fatal shores of West Africa where for four years he was stationed; in the Baltic Sea during the Russian war; in that fearful convulsion, the Indian mutiny of 1857, which threatened the integrity of the British Empire; in the sanguinary onslaught and attack upon the Paku forts on the Peiho river, on which occasion he was specially mentioned in despatches, and for his care of the wounded—being then the principal medical officer of the expedition—he received the special thanks of the Commander-in-Chief. In all these

eventful periods of his career, he proved himself equal to, and in touch with, his position. His medical history of H.M.S. *Chesapeake* in India and in Arabia in 1857-58 won for him the Blane gold medal and placed him intellectually high in esteem. Besides this blue-ribbon of the medical department of the Navy, he received a medal of Honour and Merit from the French Maritime Sanitary Congress for his writings and works on Naval Hygiene. These honours, together with the medals that were conferred upon him by the British Government, were valued by him chiefly as expressive marks of the esteem in which the work he had performed was held.

It was in 1844 that he was selected as medical officer in charge of H.M.S. *Pagoda*. This expedition, under the command of Commander (afterwards Admiral) T. L. Moore, was intended to supplement the work so ably done by Sir James Ross in the *Erebus* and *Terror* during the years 1839-43 (a work which Sir Roderick Murchison characterised as "the most remarkable and important expedition of discovery since the days of Cook"). In the two months engaged in this duty—of which Dr. Dickson has left record in his work, *The Antarctic Voyage of H.M.S. Pagoda*—the *Pagoda* completed successfully one-third of the Antarctic circumference between the meridians of Greenwich and 110° east longitude, and between the latitudes of 60° and 70° south—that is to say, the Polar Sea directly south of the Indian Ocean for a distance of more than 5,000 miles.

In a letter to the Editor of the *Standard*, dated 10th September 1886, Dr. Dickson writes as follows :—

"Proceeding from the Cape of Good Hope to 60° S. lat. we sailed south-east, and traversed a hundred degrees of longitude, seldom without seeing ice, and most often in a sea crowded with enormous icebergs, which appear to be more numerous and of greater dimensions, especially between 80° and 100° E. long., than in any part of the world. We crossed the Antarctic circle in long. 31° E. on the 8th February 1845, but had to recross it on the 15th, having been brought up in lat. 68° by heavy pack ice, forming an impenetrable wall forbidding all further approach to the Pole. Winds also which were fair to the northward of the circle were now adverse, rendering sailing slow and hazardous; and in calms the sea-water temperature of 28°+0. Fahr. and the pancake ice around the ship gave warning of the extreme peril of our position. Gales and snowstorms were of frequent occurrence the nearer we approached the land,* which doubtless exists throughout the Antarctic circumference,

* M. Borchgrevink landed on that continent 22nd February 1895—at Cape Adair—and so proved its existence.

but which except in open summers is probably very rarely accessible. We saw large icebergs as far north as 53° S. lat. in the longitude of the Cape of Good Hope, and as 51° S. lat. in the longitude of West Australia. But they are most abundant from the 60th parallel to the 65th. The most

lofty we saw was about one hundred and eighty feet high; the largest were three miles long, with an average height of one hundred feet. Their sides were precipitous and washed by a tremendous surf."

How delighted would he have been had he lived to have shared the pleasures, joys, and anticipations which possessed the breasts of the many celebrated and distinguished members of the International Geographical Congress assembled in the Hall of the Imperial Institute in the latter days of July and the first days of August of this year. For his pride and his love of country were centred in the hope that the triple cross of England's flag would one day float proudly over that land, now covered by the mighty cap of ice, which like a miser hoards and hides beneath its selfish grasp many mysterious and hitherto unknown secrets, blotting out with its icy mantle a tract of land covering more than 3,000,000 of square miles.

Well and truly would he have warmed at the inspiring words of hope expressed by the President of the Royal Geographical Society in his admirable address to the assembled geographers on the eventful evening of the 26th July 1895.

He was nominated in 1862 (on the lamented death of Dr. McWilliam) Medical Inspector of Customs, and though high on the list of promotion in the Naval Service, he accepted the post. For much and valuable work in connection with the legislation for the improvement of the Mercantile Marine, he received the thanks of the Board of Trade, and on several occasions special thanks were given him by the Board of Customs for services to that department. The care and attention he gave to the quality of the lime juice and the suggestions offered by him, saved many a ship from scurvy, "that dreadful plague of the sea and the spoyle of mariners".

Early in his civil career he joined the Metropolitan Counties Branch of the British Medical Association, of which he was President during the year 1885, and was for many years honorary treasurer to the branch.

He was also a Surgeon Lieut.-Colonel in the rifle volunteer force.

He served on the Committee on Cholera and Quarantine, 1865, and gave evidence to the Admiralty Committees on enthetic disease, on the Arctic Expedition, 1877; on scurvy and other hygienic subjects.

He was elected President of the Epidemiological Society, and occupied the presidential chair from 1885 to 1887,

being for that time "its directing mind, its guiding spirit, and its controlling adviser". He was proud of his position, esteemed it as an object of laudable ambition, and gave to it a prestige and éclat.

Earnest in all things, he entered heart and soul into all he undertook. Of a frank, open, and candid nature, he made himself welcome everywhere. Possessed as he was of that spirit of religious reverence which characterises the man whose life is passed between two immensities,

"Quo cumque adspicias, nil nisi pontus et aer",

he showed the influence that spirit had in the exercise of his thoughts and actions. He used that sense with intelligent discernment, and with such reverence and respect as to cause it, as a placid stream, to run through his life, a guide and rule of his conduct. He felt the import of the Basque proverb,

"Othrizeu estaquieua Jaincoiri Berraio itasoari"

(the man who knows not how to pray to God should go to the sea to learn), and deeply profited by it.

Of refined taste, both literary and æsthetic, he cultivated that taste with method and system, and when imparting the information derived from his extensive reading, observation, and love of art (all which he guided with thought and an educated mental training), his store of knowledge issued in words rounded in pleasing phrase, and in periods gently and ably cadenced. It was not in the blaze of sunshine, but in the calm quiet routine of a life of duty, and by the bedside of his patient, that he won for himself universal esteem; and men, who thus knew him, spoke of Walter Dickson with affection and approval, and as of one whose example they longed to imitate, and in whose footsteps they would willingly tread. One who knew, loved, and valued him, said of him that "he was as near perfection as possible". In his professional observations and experience, he followed the essentially useful and philosophic precept inculcated by Morgagni:

"Nulla est alia pro certo noserudi via, nisi quam plurimas et morborum et dissectionem historias, tum aliorum, tum proprias collectas habere, et inter se Comparare." (*De sedibus et causis morborum.*)

In all he undertook, in all he did, he was actuated by a stern sense of duty; and in all he witnessed and observed, he bowed down and confessed—with deep reverence—the manifestations of an Almighty Majestic Design.

The Ordinary Meetings of the Society are held at

11, CHANDOS STREET, CAVENDISH SQUARE,

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THE CHAIR IS TAKEN AT EIGHT O'CLOCK P.M. PRECISELY.

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Non-Resident Members are liable to an annual payment of half-a-guinea. They have the privilege of attending meetings, and are entitled to receive the publications of the Society issued during the year for which their subscription is paid; but they are not entitled to vote at meetings of the Society nor to hold office in it.

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The Transactions for the Sessions 1859-60, 1866-67, *and* 1867-68 *are out of print. With these exceptions, those for the Sessions from* 1860-61 *to the last Session inclusive, may be obtained from the publishers,* Messrs. SHAW AND SONS, FETTER LANE, E.C. *Prior to* 1859-60, *the Transactions were published in the* "SANITARY REVIEW." *The issue of a New Series of the Transactions was begun in* 1882.

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